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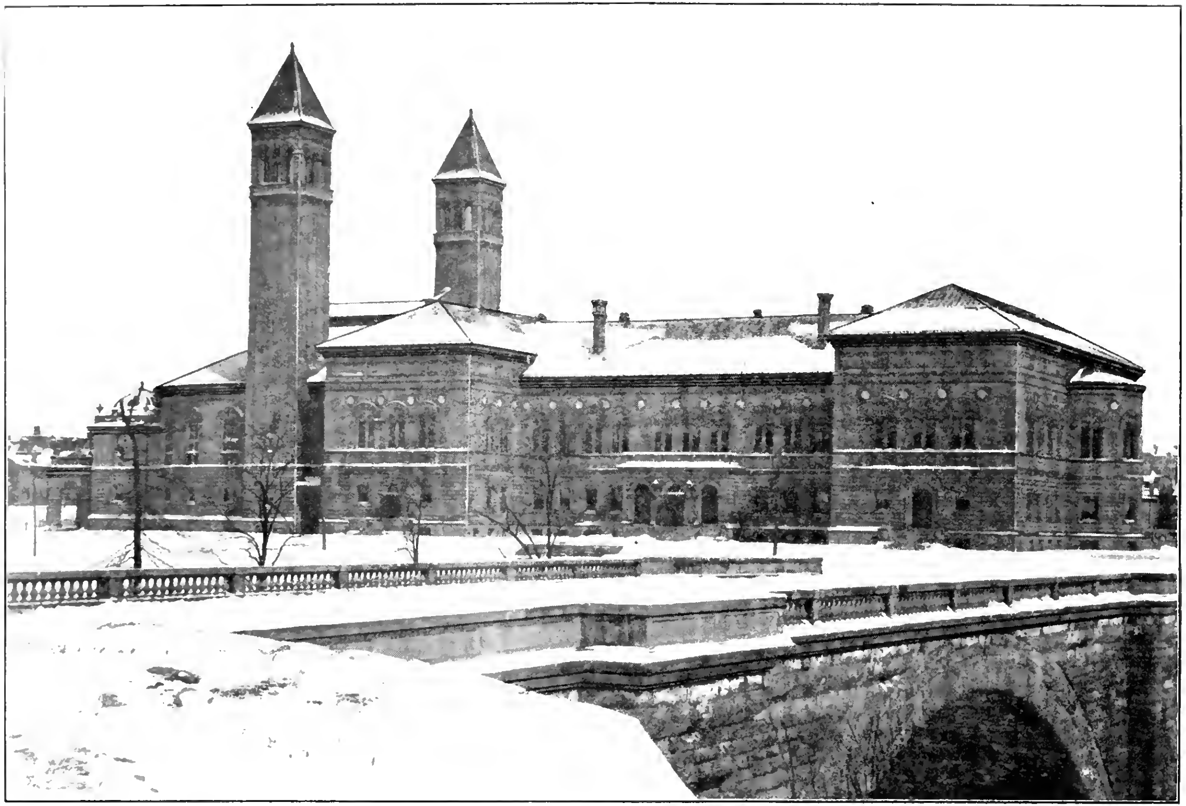


THE CARNEGIE INSTITUTE, PITTSBURGH, PA.

THE

POPULAR SCIENCE

MONTHLY.

MAY, 1901.

THE CARNEGIE MUSEUM.*

By W. J. HOLLAND, LL. D.,
DIRECTOR OF THE MUSEUM.

IT was a glorious summer day. The sunlight gleamed through the trees, which covered the mountain-top. Checkers of light and shade wove themselves upon the fern-clad soil. Seated upon the trunk of a fallen tree the man whose name to-day is borne by scores of institutions, which his more than princely benevolence has founded, talked to a friend in relation to his plans for the great city, the history of the growth of which is closely linked with the story of his own wonderful career. "The Allegheny Library will before long be nearing completion," he said, "and the time is approaching to execute my designs for Pittsburgh. In my original offer I agreed to give Pittsburgh a quarter of a million of dollars with which to build a library, but I mean to enlarge my gift, and make it a million. I have given Allegheny a library and a music-hall. I wish to do as much for Pittsburgh. The library idea is central. My convictions on that subject are established. But I wish to do something more than to found a library in Pittsburgh. I am thinking of incorporating with the plan for a library that of an art-gallery in which shall be preserved a record of the progress and development of pictorial art in America, and perhaps also of making some provision for advancing knowledge among the people through the addition of accommodations for the various societies which in recent years have struggled into existence among us. These societies deserve to be encouraged. I mean the Art Society, the Botanical Society of Western Pennsylvania, the Microscopical Society of Pittsburgh, and

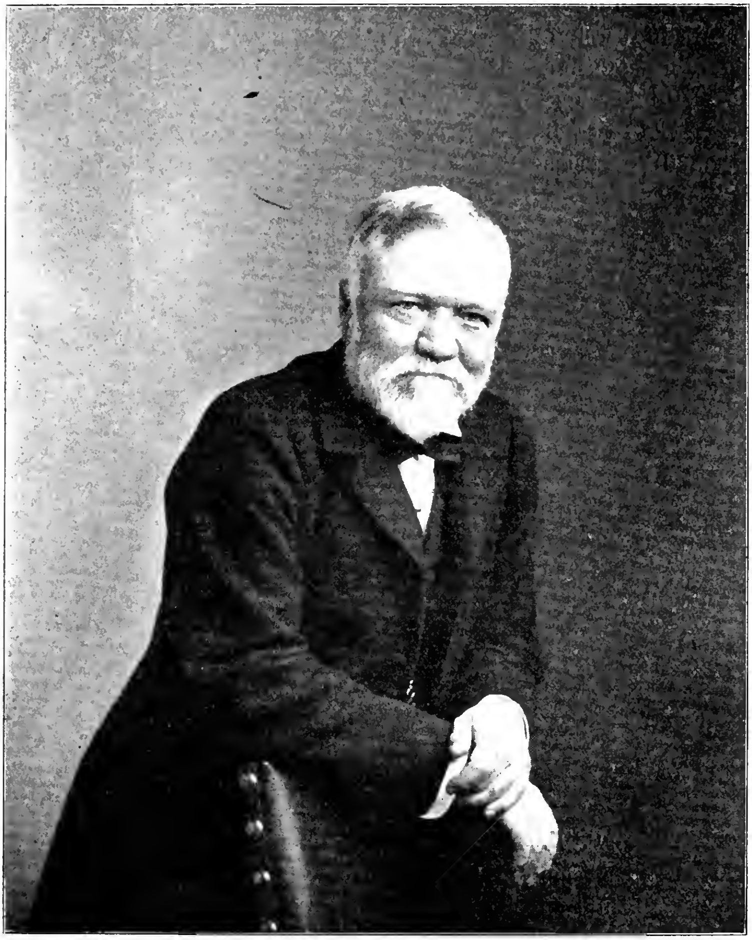
* Prepared at the special request of the Editor of the POPULAR SCIENCE MONTHLY.

all those other societies. Get them to join their forces and unite to form one society—call it the Academy of Science and Art of Pittsburgh, if you please—and I will furnish accommodations for them when I come to build the library in Pittsburgh. We can treat with one central organization better than with half a dozen different societies. Some of these societies are forming collections of books, historical objects, natural history specimens. These things ought to be kept in fire-proof quarters. That is another point on which I am sound. I believe in fire-proof construction. There are your butterflies, for instance. Such collections should not be exposed to the risk of fire. When I build the library I will provide a good place in which to keep them." So the plan was unfolded and its outlines sketched while the leaves rustled and the birds sang overhead.

Nine years took their flight, and at last the dream was transmuted into stone and marble. The structure which the fancy had outlined stood revealed in the beauty of architectural form and the still greater beauty of definite purpose and usefulness. When on November 5, 1895, the edifice was formally presented to the city of Pittsburgh by its donor it was found to contain accommodations for a great central library, with provision for the administration from this center of a number of branch libraries, for the erection of which ample funds had been provided. Under the same roof was a music-hall, one of the most perfect of its kind in the United States, an art gallery of noble proportions and, forming the southern wing of the great building, the Museum, on the first floor of which was provided a spacious lecture-hall adapted to the uses of the learned societies, which, in pursuance of the suggestion of the founder, had been merged into the Academy of Science and Art of Pittsburgh.

Prior to the opening of the building arrangements were made by the Academy of Science and Art to gather together a collection of objects suitable for exhibition in a museum. The Curator of the Academy, Dr. Gustave Guttenberg, labored strenuously to place the material in proper order, and was aided by his associates, who freely gave their time and generously contributed of their means to make the exhibition worthy of the occasion. The result revealed, as all such attempts in our great cities are certain to show, how large an accumulation of really choice specimens exists in the hands of individuals who are possessed of artistic and scientific tastes. Ethnological, mineralogical and zoological collections of no small merit were rapidly brought together from the homes of scores of citizens, whose interest had been awakened, and the collections in the possession of the Western University of Pennsylvania were laid under heavy contribution to fill up any gaps, which required for the time being to be closed, in order to replenish the cases and dress the halls.

When, on November 5, 1895, the edifice was thrown open to the people, the splendid generosity of the gift produced a profound impression, but the gratitude which was felt was converted into amazed thankfulness when the donor announced to the large audience which filled the auditorium that it was his intention to supplement his gift by the



ANDREW CARNEGIE.

bestowal of an additional million of dollars as a permanent endowment, the annual income to be used in promoting the interests of the Art Gallery and the Museum. The custodianship of the endowment fund was committed to a Board of Trustees, consisting of the gentlemen who were already vested with the care of the building, and eighteen others, who were named by the donor because of their interest in those things

which tend to promote scientific and æsthetic culture. The formal title assumed by this body was 'The Board of Trustees of the Carnegie Fine Arts and Museum Collection Fund,' subsequently changed to 'The Trustees of the Carnegie Institute.'

The announcement of this gift and the conditions which were to govern the trust necessitated a change in the administration of the affairs of the Museum. The control of the Museum and the collections contained in it was transferred from the Academy of Science and Art to the newly appointed Trustees of the Endowment Fund, the Academy of Science and Art engaging to cooperate with the Trustees and to apply the revenues in their possession, derived from the annual dues of the membership, to the maintenance of courses of popular lectures in the hall of the Museum.

GUSTAVE GUTTENBERG.

The immediate oversight of the Museum was vested by the action of the Trustees in a committee of eight, including *ex officio* the President of the Board. The committee as at first constituted consisted of the following gentlemen: C. C. Mellor, Chairman; Samuel Harden Church, Litt. D., Secretary; W. N. Frew, Esq., President of the Board; Rev. A. A. Lambing, President of the Western Pennsylvania Historical Society; Hon. H. P. Ford, Mayor of Pittsburgh; John A. Brashear, Sc. D.; Josiah Cohen, Esq., and W. J. Holland, LL. D., Chancellor of the Western University of Pennsylvania.

Unfortunately, the hand of death removed the man who would have been the first choice of the Trustees for the important position of Director of the new Museum. Professor Gustave Guttenberg died in

Vienna before a full organization of the committee on the affairs of the Museum had been effected. The first act of the Board was to purchase from his widow the beautiful collection of minerals which he had made, and which had been one of the attractive features of the opening exhibition of the Museum.

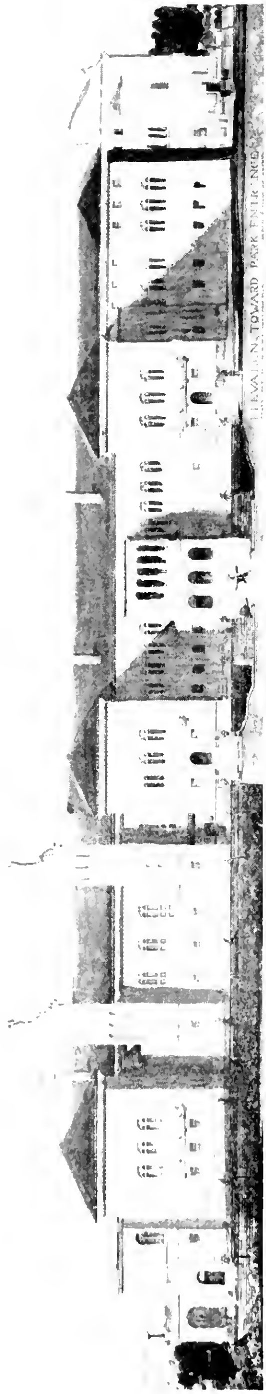
W. N. FREW, PRESIDENT OF THE BOARD OF TRUSTEES.

C. C. MELLOR, A.M., CHAIRMAN OF THE COMMITTEE ON THE MUSEUM.

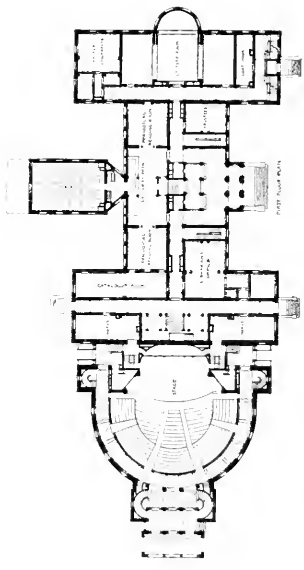
S. H. CHURCH, LITT. D., SECRETARY OF THE BOARD OF TRUSTEES.

After several experiments in administrative arrangement, which were not wholly satisfactory, in the spring of 1898 Dr. W. J. Holland was elected as the Director of the Museum. This relationship still continues.

The Museum at present occupies six halls, which are devoted to

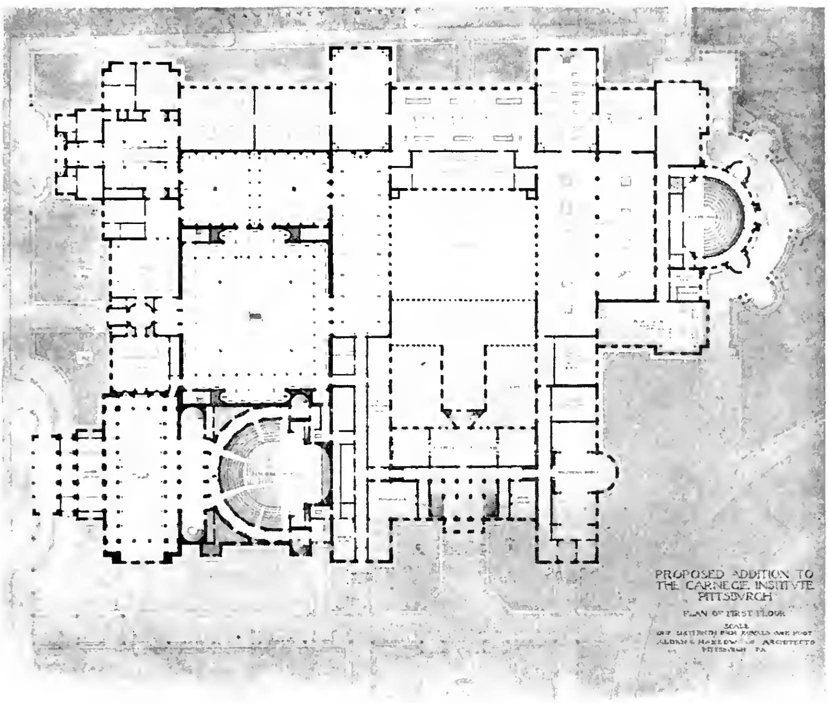


Side Elevation of the Proposed New Buildings of the Carnegie Institute.



Ground Plan of the Present Carnegie Institute.

purposes of display, and seven rooms which are used as laboratories and offices. Three of the exhibition halls are situated on the second floor of the building and three upon the third. Two of the laboratories are situated on either side of the lecture-hall on the first floor, and the three remaining laboratories are in the basement of the building. The floor space available for the display of the collections amounts, at the present time, to a little more than twelve thousand square feet. The floor space devoted to laboratories is five thousand square feet. The lecture-hall will comfortably accommodate about six hundred persons.



GROUND PLAN OF THE PROPOSED ADDITION TO THE CARNEGIE INSTITUTE.

The walls of a museum are to its contents what the frame is to a picture. The generosity of the founder provided at the outset a beautiful edifice under the roof of which to assemble the collections which it was destined to contain, but he did not forget to provide for what after all is the museum itself, and has from year to year supplemented the income derived from his original gift of a million of dollars by the purchase of collections, which he has himself selected, or by placing at the disposal of the Director of the Museum funds with which to make special collections.

The growth of the Museum and the related departments of the Institute has been so rapid, and the usefulness and popularity of the entire undertaking has been so great, that the founder has found himself constrained to again provide for further enlargement, and in the fall of the year 1899 and the spring of 1900 preliminary plans for extension were prepared, which subsequently were approved by Mr. Carnegie. These plans contemplate the ultimate expenditure of $3,600,000, in new construction, greatly enlarging and perfecting the facilities of the Museum, the Library and the Art Gallery. When these plans are executed the city of Pittsburgh will have an institution second in its importance to no other of like character in the New World, and surpassing many of the famous institutions of Europe in the provision made within its walls for promoting a knowledge of literature, science and art.

Inasmuch as Pittsburgh is located in the very heart of the Appalachian region, it was in the beginning determined among other things to make the collections acquired by the institution as thoroughly illustrative of this region as possible. Accordingly much effort has been expended in endeavoring to obtain specimens illustrating the geology, the mineral resources, and the flora and fauna of the region of which Pittsburgh may be said to be the metropolis. By the gift of the large herbarium of the Western Pennsylvania Botanical Society, to which extensive additions have been made, the flora of the region is already well represented. The fauna is also represented by collections which are extensive and rapidly growing. Almost all the mammals and birds known to exist in Western Pennsylvania are contained in the collection, and through the diligence of those in charge of the department of ornithology several species not heretofore known to occur within the limits of Pennsylvania have been added to the faunal list. The collections representing the insect life of the region are great. Extensive research is going on in every direction, and it is hoped ultimately to amass and bring together representatives of every form of life, whether animal or vegetable, known to occur in the upper valley of the Ohio. Collectors have been sent out who have extended their labors over the whole western half of the State, from Erie to the southern boundary, and westward into eastern Ohio, and southward into West Virginia. It no doubt will require many years finally to complete the biological survey of this extensive region, but a very satisfactory beginning has already been made. Side by side with the work done in the department of biology much work has been done in gathering together ethnological and historical material, the former throwing light upon the aboriginal inhabitants of the territory, the latter serving to illustrate its development since occupied by civilized man. The industries of the region likewise have claimed

attention, and important industrial exhibits have been formed, showing the development of commerce and manufactures in western Pennsylvania.

It is far, however, from the purpose of the Trustees to restrict the Museum to the work which has just been outlined. The whole field of research is before them, and already very large accumulations of material from distant parts of our own continent and from foreign lands have been brought together. The collections already in the possession of the Museum may be approximately classified as follows:

	Species and Varieties.	Specimens.
Minerals	400	4,000
Geological Specimens		1,000
Botany (recent species)	17,000	100,000
Botany (fossil)	150	1,200
Paleontology (invertebrate)	500	2,400
Paleontology (vertebrate)	160	3,500
Porifera, Echinoderms, etc	500	1,250
Mollusca	9,500	100,000
Crustacea	100	2,000
Arachnida	300	1,200
Myriapoda	50	1,200
Hymenoptera	1,250	4,000
Lepidoptera	20,000	300,000
Diptera	1,000	5,000
Coleoptera	20,000	275,000
Hemiptera	750	4,000
Orthoptera	400	1,600
Neuroptera	300	1,200
Fishes	500	1,800
Reptilia and Batrachia	150	1,750
Birds	1,200	9,000
Mammals	300	1,050
Total	74,510	822,150

The foregoing table shows that the collections representing the various classes in the vegetable and animal kingdom are somewhat unequal in the matter of extent. The assemblage of shells is already large because of the acquisition by the Museum of several considerable collections, one of them made in South America by Mr. Herbert H. Smith; the other by the late F. R. Holland, which contains a large number of species represented by cotypes and specimens autographically labeled by Adams, Anthony, Bland and other early American conchologists. This collection at the time of its acquisition by the Carnegie Museum contained over six thousand species and is especially rich in West Indian terrestrial mollusca. The collection of Lepidoptera is also exceedingly rich in species, as well as specimens, containing as it does, the entire collection of Mr. W. H. Edwards, the author of the 'Butterflies

of North America,' with almost all his types, as well as many types and paratypes obtained from Boisduval, Tryon, Reakirt, Henry Edwards, S. H. Scudder, and Dr. Herman Behr. The collection also includes the entire collection made by Theodore L. Mead, the types of all species described by the present Director of the Museum, numerous types of species described by Lord Walsingham, E. L. Ragonot, Arthur G. Butler, Sir George Hampson, William Doherty, Dr. Henry Skinner and others, and cotypes of a multitude of species obtained from various authors in different parts of the world. There are over three thousand types and cotypes in the collection of Lepidoptera. The collection is particularly rich in North American, Japanese, Indian and African species. The Knyvett collection of Indian Lepidoptera was purchased

HENRY ULKE.

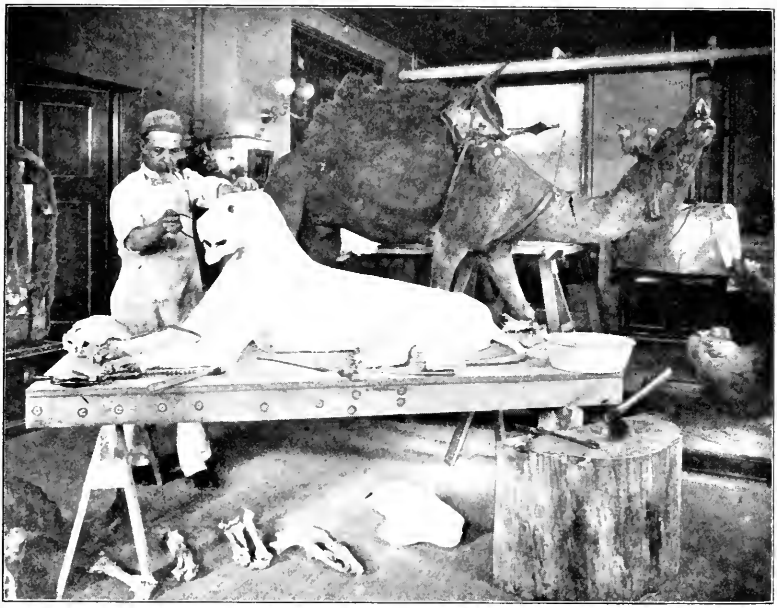
FREDERICK S. WEBSTER.

by Mr. Carnegie some years ago. It contains over three thousand species of Indian Lepidoptera, mostly represented by large series of specimens. Large portions of the collections made by Doherty in India and in the Malay Archipelago are also here. The micro-lepidoptera of Japan, collected by the late Henry Pryer, of Yokohama, are also incorporated in the collection, having been purchased in 1887, a year before the lamented death of Pryer. Latterly extensive additions have been made in the form of material secured from various localities in Africa, Mexico and Central America, and from the continent of South America, the latter principally through the labors of Herbert H. Smith.

The assemblage of coleoptera, comprising among other things the collections of the late Dr. Hamilton, of Allegheny, and of Henry

Ulke, of Washington, D. C., is one of the largest and most perfect collections of the beetles of North America in existence. It is rich in types and cotypes, several thousand species being thus represented. In addition to the North American collections of coleoptera, there are vast accumulations of material from other parts of the world, especially from Africa, tropical America and Japan. The collections in other orders of insects represent mostly North American material, though in every order there is more or less exotic material.

In the ornithological collections North American species preponderate. There are about nine thousand specimens of birds in the pos-

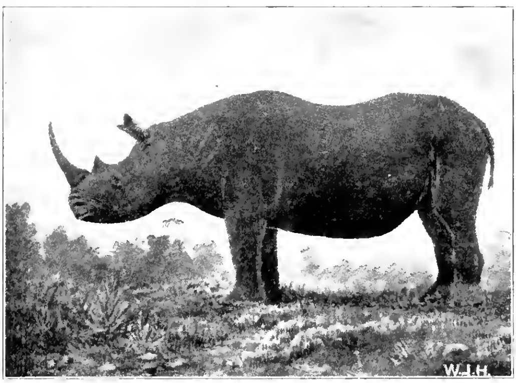


A PEEP INTO THE TAXIDERMIC LABORATORY.

session of the Museum, as the result of the accumulations made during the last three years. Fully three-fourths of these belong to the native series. Of the species of birds known to occur within the State of Pennsylvania almost all are represented. Great skill and taste have been displayed by Mr. Frederick S. Webster, the chief preparator in the department of zoology, in the composition of a number of very life-like and attractive groups representing some of the more remarkable as well as the commoner forms of bird-life found in America. The groups of flamingoes, Californian condors and brown pelicans are large and effective. One of the most striking compositions is that of the famous setter-dog, 'Count Noble,' flushing a covey of quails. 'Count Noble,'

the progenitor of many of the finest dogs of his race in America, breathed his last in Pittsburgh, and by happy fortune his skin was preserved and came into the possession of the Museum. Many of the smaller groups of birds delineate accurately the habits of the more familiar species and are accepted as masterpieces of the taxidermic art.

The mammals are represented by small, but important, collections. One of the recent acquisitions is that of a specimen of *Rhinoceros simus*. This large mammal, which is, with the exception of the elephant, the largest of terrestrial quadrupeds, is believed to be on the verge of extinction. A few years ago the Hon. Cecil Rhodes secured a specimen by purchase, which he presented to the South African Museum at Cape Town. Another was secured by the British Museum, a third specimen was acquired by the Hon. Walter Rothschild for his private collection at Tring, and a fourth was purchased by the Imperial Academy of Sciences at St. Petersburg. The specimen just acquired by the Carnegie Museum is the fifth to be preserved as a memorial of its rapidly vanishing race, and is the only specimen known to exist in the New World.



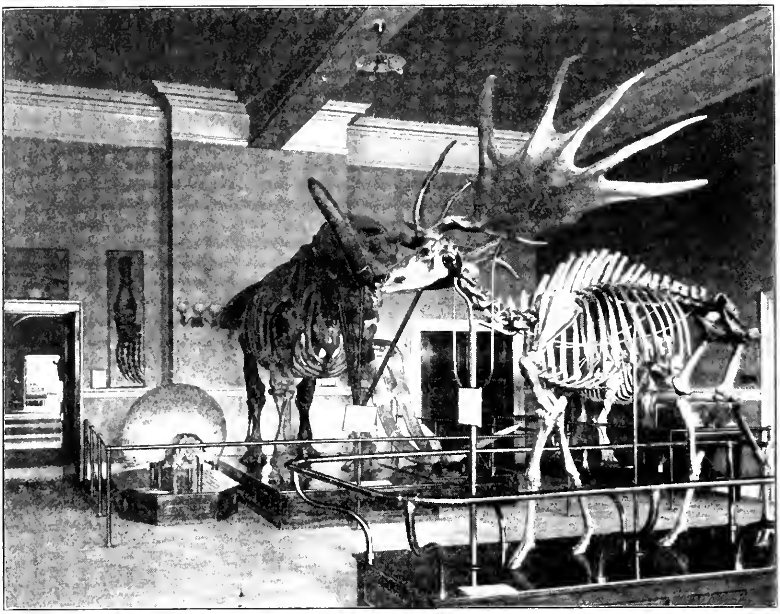
RHINOCEROS SIMUS BURCHELL.

One of the most fruitful departments of activity in connection with the Museum is presided over by Prof. J. B. Hatcher, the famous explorer and paleontologist. Mr. Carnegie has long realized the importance of paleontology as throwing light upon the evolution of species, and in the spring of 1899 provided a special fund for research in this direction. The results have been most satisfactory, when regard is had alike to the number of the important discoveries which have been made and the beauty and perfection of the specimens which have been obtained. It is well known that the evolution of the horse took place in North America. The discoveries of Professor Hatcher made in 1900 show that in all probability in like manner the rhinoceros was

evolved from a primitive form upon the same theater of zoogenic energy. The most striking objects in the paleontological section, from a popular standpoint, are the huge dinosaurs from the Jurassic beds of Wyoming and Colorado. The most perfect specimen of *Diplodocus longus* Marsh known to exist anywhere was secured in the summer of 1899. This huge, lizard-like quadruped was about seventy feet in length from the tip of the nose to the end of the tail, and stood fully fifteen feet in height at the hips. Six skeletons of Brontosaurus, a still huger monster, have also been discovered and collected. In no instance were these skeletons complete, but enough material has been secured, it is believed, to admit of the restoration of a composite skeleton of Brontosaurus as well as that of Diplodocus. Within the limits of a brief sketch it is impossible to speak at length of the collections brought together in the section of paleontology, but it is worthy of note that the Museum contains the largest specimen of the Mastodon known to exist, and with the single exception of the 'Warren Mastodon,' which

GORILLA, SPECIMEN COLLECTED BY REV. A. C. GOOD. PH. D., AT KANGWE, OGOVE RIVER, WEST AFRICA.

IN THE PALEONTOLOGICAL LABORATORY: SETTING UP THE HIND LEG OF A BRONTOSAUR.



A Corner in the Hall of Paleontology.

A Glimpse into the Hall of Ethnology. North American Indians.

is now hidden away in Boston and invisible to the public, probably the most perfect specimen in any Museum.

A good foundation has been laid for the development of the section of archeology. The aboriginal races of America as represented by the mound-builders of the Ohio Valley, the cliff-dwellers of Arizona and the ancient populations of Mexico are in evidence in many ways. One of the latest acquisitions has been a series of reproductions of the carvings in stone preserved in the National Museum of Mexico. These

THE MOKI SNAKE DANCERS. GROUP MODELED BY T. A. MILLS.

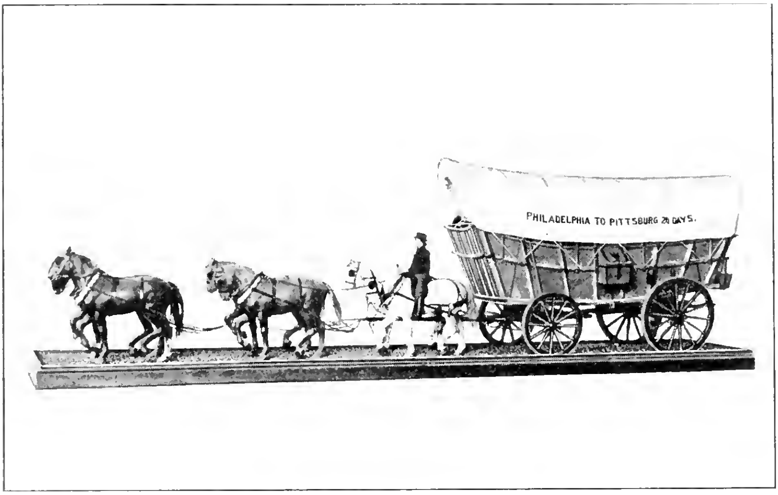
reproductions were made at the expense of Mr. Carnegie and are a duplication to the city of Pittsburgh of the gift made recently to the city of New York by the Duc de Loubat, and preserved in the American Museum of Natural History in Central Park.

The surviving Indian races of North America are represented by an extensive series of models and groups made by Mr. T. A. Mills, the well-known sculptor, all being clothed in characteristic costumes, selected with great care to represent the manners and customs which prevail among them. Besides, there are extensive collections of imple-

ments and utensils in use among these various tribes. The same remark holds good of the Esquimaux of Alaska.

The archeology of the old world has not been forgotten, and already, partly by gift and partly by purchase, considerable assemblages of specimens throwing light upon the ancient civilizations of southern Europe, Egypt and Asia Minor have been secured. The collection of reproductions of the famous Neapolitan bronzes, presented by Mr. Carnegie, duplicates for Pittsburgh the same series now in the Metropolitan Museum of Art in New York. The collections annually obtained through the Pittsburgh Branch of the Egypt Exploration Fund constitute an ever-growing series of high valuable and important objects.

The development of the domestic and industrial arts in America from the first colonization to the present is illustrated by a series of

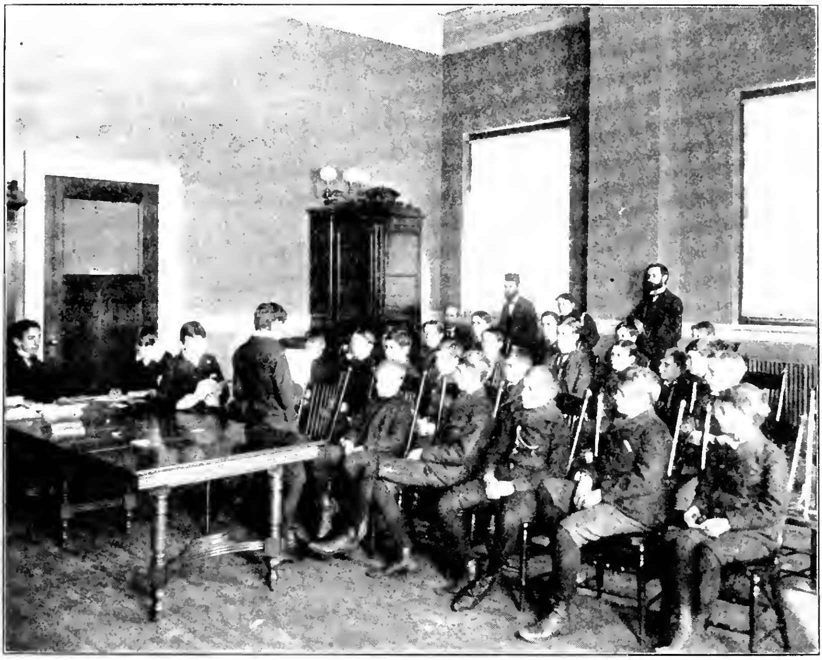


Model of Conestoga Wagon, made by Wilson Banks and T. A. Mills.

collections to which additions are being rapidly made. The evolution of methods of transportation is shown by a long series of models constructed by Mr. Wilson Banks, Mr. T. A. Mills and others. This series is in part a reduplication of specimens now in the U. S. National Museum at Washington.

The end for which museums exist is not simply the acquisition and preservation of curious and instructive specimens. The great object which such an institution should ever keep in view is the diffusion of knowledge. The management of the Carnegie Museum has realized this from the very inception of its work. Care has been devoted to the proper arrangement, display and labeling of those parts of the collections placed on view. The late G. Brown Goode once said in substance,

'A good museum is a collection of good labels, illustrated by carefully selected specimens.' Much time and thought has been expended in the endeavor to tell to the observer in simple and intelligible language the truth which the collections are intended to illustrate. In order to enlist the interest of children a series of prizes has been annually offered to the pupils in the high-schools and the upper classes in the grammar-schools of the city of Pittsburgh. The prizes are awarded to those who shall write the best essay upon some subject illustrated by the collections contained in the Museum. Thirty-eight prizes,



A SECTION OF THE ANDREW CARNEGIE NATURALISTS' CLUB IN SESSION.

ranging in value from $25 to $2, were offered in 1900. Eight hundred and forty-three essays were submitted in competition. The decision of the awards is made by a committee of judges consisting of thirty of the most cultivated ladies and gentlemen of the city, among them a number of eminent clergymen, lawyers, editors and authors. The plan requires on the part of the contestants a personal visit to the Museum and the study of the collections. During the month preceding the close of the contest the Museum was at times crowded by eager throngs of intelligent boys and girls armed with note-books and pencils. The delicious compliment of imitation has been paid to the Carnegie Mu-

seum since this plan was adopted by several kindred institutions in America and in Europe.

A further effort to interest and instruct the youth of the community has led to the formation of a society known as the Andrew Carnegie Naturalists' Club, which consists of between two and three hundred young people who meet every other week on the afternoon of Saturday in the lecture-hall of the Museum and hear lectures, often illustrated by specimens and the stereopticon, and who read papers upon subjects of interest. During the summer months the club makes excursions in the neighborhood, and the various subdivisions receive practical instruction from the staff of the Museum in the art of collecting and preserving specimens of plants and animals.

The wider diffusion of knowledge among scientific men and institutions is provided for by the publication of the 'Annals' and 'Memoirs' of the Museum. The former appear in octavo form, the latter in quarto. This series of publications began with the first month of the twentieth century, and it is hoped will not end so long as the centuries run their course.

THE AURORA AUSTRALIS, AS OBSERVED FROM THE 'BELGICA.'

BY DR. FREDERICK A. COOK.

IN the literature of the still unknown phenomena of polar auroras, deductions have been based almost entirely upon observations of the aurora borealis. So little has been known of the south pole and of its terrestrial and celestial surroundings that the aurora australis has been omitted in the upbuilding of auroral science. From the observations of the Belgian expedition and from the reports of forgotten previous explorers, it would seem that the auroras of the south are not so brilliant or so varied in form and character as those reported from the north. Auroras in brilliant colors and in fantastic heavenly drapery are indeed rare in the regions invaded by the 'Belgica.' It should, however, be remembered that the austral phenomenon is but vaguely known. The 'Belgica's' drift covers but a small space in the great unknown area about the south pole. Nearly eight million square miles, a region as large as all North America, is still a blank under the Southern Cross. At other points within this area the aurora may appear differently. Such a condition obtains in the arctic. Nordenskiold, viewing the northern lights from the sea north of Siberia, saw displays almost exactly like those seen from the 'Belgica' south of the Pacific, but Peary and all the explorers who wintered on the Greenland side of the geographical pole have described auroras in vivid colors and fantastic forms.

The antarctic continent, which is just the region from which the southern lights can best be studied, is still unexplored, and most of it is inaccessible. If we can judge from similar latitudes in the north, the edge of this great continent of ice is an ideal latitude for effective observatories, and no doubt future explorers will seek favorable locations from which to observe this curious phenomenon.

The inhabited parts of Australasia, southern South America and Africa are too far north to offer a good station to study these phenomena. There are no convenient land projections in the antarctic, like Siberia, Norway and Greenland in the arctic, where comfortable stations could be established. From this it results that few careful studies of the austral aurora have been made. The great restless, ice-encumbered sea which sweeps around the south polar area is not favorable for such observations. Captain Cook, who, during three years, circumnavigated the globe in high latitudes, barely mentions the aurora. Ross, Wilkes and d'Urville were in the ice regions only during the days of summer, when auroras were seldom visible.

The early sealers, who in the first quarter of the last century invaded the lonely southern seas, rarely mention the aurora. From the observations of the sealers and the early explorers it would seem as if we should have a fair idea of the austral auroras, but all antarctic voyagers have devoted most of their time to skirting the edge of the pack-ice, where the sky is almost constantly veiled by a haze of either fog or snow. The fact that the pioneers in the far south have seen so little of the aurora has led to the impression that the phenomenon there is feeble, but such an impression should not be favored until we have a more thorough series of observations.

Ross and Wilkes saw a few vivid displays of draped auroras, tinged with prismatic colors, but from the 'Belgica,' which was the first vessel to spend a winter in the antarctic, we saw few colors, seldom draped, and only rarely fleeting rays which spread over a large part of the sky. Below is a table of the observations recorded by Henryk Arctowski, the meteorologist of the Belgian expedition:

TABLE OF AURORAS OBSERVED ON BOARD THE 'BELGICA' DURING THE WINTER OF 1898.

	March.	April.	May.	June.	July.	August.	September.
1	—	—	—	—	—	L.	—
2	—	—	—	—	—	—	Ad.S.R.V.P.
3	—	L.	A. V.	—	—	—	—
4	—	—	—	—	—	—	—
5	—	—	—	—	—	—	A.
6	—	Ad. R.	—	—	—	—	—
7	—	—	—	—	—	—	—
8	—	—	—	—	L.	—	L.
9	—	—	—	—	L.	—	S.A.R. Ad.
10	—	A.S. Ad.		A.	L.	—	R.S.A. Ad.
11	A.	L.	—	—	L.	—	—
12	A.	—	—	—	L.	—	—
13	—	L.	—	S.A.F.	L.A.S.O.	—	—
14	A.P.W.C.	A.S.O.V.R.P.	—	L.	—	—	—
15	—	A.S.Ad.R.	—	L.	L.	—	—
16	—	—	L.	—	—	L.	—
17	—	—	—	—	L.	—	—
18	—	—	—	—	—	L.	—
19	Am. V.P.	—	—	—	—	Ad. S.	—
20	A.R.V.	—	A. S.	—	—	L.	—
21	—	L.	L.	—	L.S.A.	—	—
22	—	L.A.	L.	A.S.R.	A.S.	—	—
23	L.	—	—	L.S.	A.	—	—
24	L.	L.		S. Ad.	L.	—	—
25	A.S.	S.A.R.	—	—	—	—	—
26	S.L.R.V.O.	—	—	—	—	A.	—
27	—	—	—	—	—	A.	—
28	L.	L.	—	—	—	—	—
29	A.S.	—	Ad.	—	—	—	—
30	—	—	—	—	—	—	—
31	A.	—	—	—	—	—	—

EXPLANATION OF SIGNS EMPLOYED.

A.=Homogeneous arc.	C.=Crown.	O.=Obscure rays.	S.=Dark segment.
Ad.=Double arc.	F.=Flames.	P.=Streamers.	V.=Wavy ribbons.
Am.=Multiple arc.	L.=Luminous glow.	R.=Rays.	W.=Curtain.

In the 'Belgica,' we had been sailing among icebergs and along the ice-sheeted coast of newly discovered lands for nearly two months before we saw the first aurora. During most of this time we were above the polar circle, where the sun, during the hours of midnight and mid-summer, sank but a few degrees behind the icy crust of the earth, leaving a twilight so brilliant that no stars were visible. The glancing rays of the nocturnal sun, which were thrown from peak to peak and from the mirror-like slopes into the heavens, made the night a scene of dazzling splendor, too bright to permit the display of the auroral light.

In the first days of March we found ourselves surrounded by a hopeless sea of ice from whose ensnaring influence we were unable to extricate ourselves. The long winter and the polar night, which no man had as yet experienced, now came over us rapidly. The sun daily sank lower on the sky and swept less of the horizon. The rose color of the snow, which made the summer nights charming, now changed into lilac. The open spaces of water between the restless ice-fields were being hidden under a weight of rapidly forming new ice, and the winds were moaning in prophetic despair of the coming blackness. We knew only too well that we were in the relentless grasp of a new monster, the Antarctic Ice King, and in his grasp we must remain until the thaw of another summer should release us. In this spirit of despondency and with considerable anxiety we searched the skies nightly for the heavenly glow of the aurora australis, which we hoped might relieve the awful monotony and soul-despairing darkness of the coming winter.

While skirting the edge of the pack-ice late in February we saw a star, the first since leaving the Cape Horn waters, and this little speck, though a sign of the long, gloomy night and of the polar winter, was hailed as a messenger from a new world. During the days which followed we watched with joy the increasing number of stars from night to night, but there was so much storm and the atmosphere was so thoroughly charged by humidity that a clear sky was rarely observed.

On the evening of March 12, 1898, we saw the first distinctive aurora. A faint arc was seen the night previous, but the light was so feeble that many of us doubted that the phenomenon was auroral. The few days which preceded were clear, sharp and cold. We had been so constantly showered with snow and sleet, so persistently held in banks of fog and so often driven to the verge of desperation by the violent storms which ever swept the pack-edge that this calm and silence was, indeed, a treat to us. On the evening of the 14th the sun sank out of a cloudless sky below the crackling, quivering ice of the sea. The temperature was —15 C. A light wind, which came out of the south, pierced the skin like needles. We were many hundred miles from the

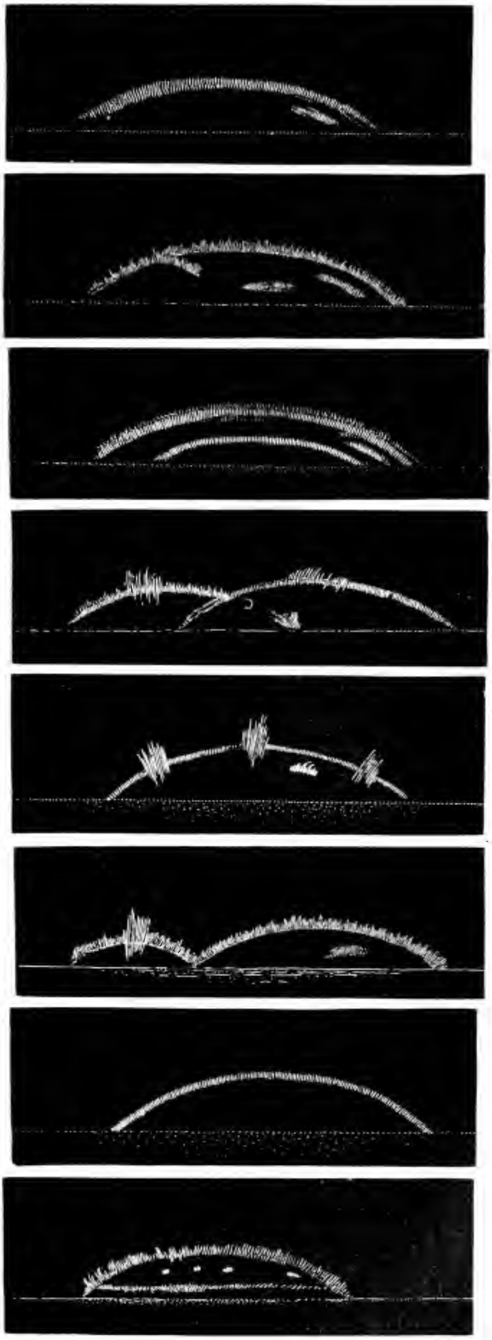
nearest land; our horizon was everywhere lined by the towering heights of icebergs which were separated by level fields of sea-ice.

Over this sheen of hard ice and soft snow there rested a haze of ice crystals which was curiously suspended in the air. As the sun sank through this haze it lost its luminous character, and before it vanished into its bed of snow it appeared as a great, distorted, rayless ball of crimson. The play of light in this icy haze is a joy experienced in no other part of the globe. Over the departing sun there remained a band of orange running into rose at the sky line and into gold at its upper edge. At the same time there rose in the east an arc of dark purple-blue, edged with orange. This is the twilight curve which is here strikingly noticeable. As the purple of twilight ascended towards the zenith, the snow westward had a delicate lilac hue, and eastward there was a bright purple-blue over everything, which finally deepened into a gobelin-blue.

At about eight o'clock the Southern Cross was clearly visible over the masts. The purple twilight curve was absorbed into the homogeneous blue of the sky. At the zenith there were a few waves of light which had the appearance of high cirrus clouds. These darted across the heavens with lightning swiftness, fading, vanishing and reappearing with augmented force each time, until at ten o'clock the phenomenon settled into a waving, luminous arc with a fringe, causing it to look like a curtain hanging low on the southern sky. Still later the fringe work gave place to a steady luminous arc, whose highest altitude was about 30°.

The evening of the 14th was also clear and calm. There was a fascinating sunset, followed by a long purple twilight. The temperature had fallen to —20 C. The glassy character of the air, the paleness of the sky and the absence of wind were to us indications of a very cold night. Such nights are always favorable to auroral displays, and we were early on a lookout for them. At about nine o'clock there appeared a bank of luminous fog in the southwest. Soon after, there rose an arc over this which was at first imperfect. Now the eastern portion was illuminated, then the western portion, and, again, only a fragment of the center was visible. So rapid were these changes that we found ourselves unable to record the fleeting forms.

Everybody was on deck or pacing the ice about the 'Belgica,' making notes and sketches of the phenomenon. The scene was such as would delight the heart of any lover of nature. The good old 'Belgica,' the home of the only speck of human life within the icy under-surface of the globe, was buried in a bed of snow which so completely covered her body that only the rigging projected. Even the masts and the ropes were encased in a heavy plating of hoar-frost and hard ice, which glittered like gems in the silvery light of the



Successive Displays of a Typical Austral Aurora seen from the 'Belgica,' March 19, 1898. Above the Arcs on this Evening there were Occasional Bands of Wavy Ribbons and Streamers.

night. As we walked around the bark in an unsuccessful effort to keep warm we saw beyond the glittering spars the glow of a great arc. This, for a time, hung steadily between the masts and then suddenly, as if the fetters which had held it together had burst, the entire southern heavens were swept by aimless bands of fleeting luminous patches.

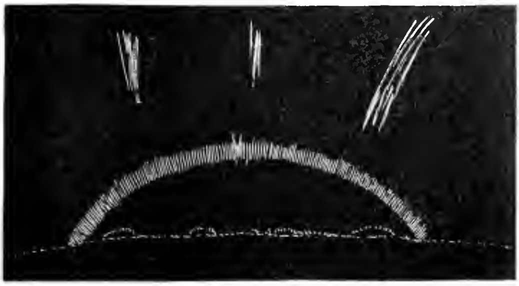
After a violent storm which lasted for three days the sky cleared again on the evening of the 19th; the wind then came in puffs with doleful wails like the moans of a dying soul. This we knew indicated that the tempest was nearly spent. At five o'clock I wrote in my log:

"5 P. M.—The storm has at last abated. It has left us so suddenly that the calm is as unexpected as it is appreciated. The barometer is steady and the temperature is falling fast. It is already 9°C., and is still falling. The scene now before us is full of new delights. The ice is spread out again, bright, soft and tinted with delicate colors. Every time the thick air and the gloomy storm clouds are brushed away, the pack, white and sparkling, has a new story to tell. It brings to us moods like a cheerful page in a sad story. Under the influence of this spell everybody is singing, whistling and humming familiar tunes; all are planning new work and nursing big ambitions. In the cabin the music-boxes are grinding out favorite music, which rings over the pack with a new joy. In the forecastle the men are dancing and playing the accordeon with telling effect. From some invisible point of the pack there comes a weird response to every discord of the music. It is the 'gha-a-ah, gha-a-aha' of the penguins. We have had a peep at the sun, and this has brought about an intoxication akin to alcoholic stimulation, and well it might, for the brief period of its visibility has been a dream of charms. The great twilight zone of purple, fringed with violet and orange and rose is rising over the east. The zenith is pale blue, studded with a few scarlet and lavender clouds, and the sun, a great ball of old gold, is sinking under the pearly rose-tinged line of the endless expanse of ice."

"8 P. M.—The ice shows signs of strong pressure from the north. Along the crevasses, running easterly and westerly, there are great lines of hummocks from four to eight feet in height. The colors of the pack are now far from the despairing monotone of yesterday. The yellow sea algæ have already fixed themselves in the new ice and make it appear ocherous. The twilight on clear nights is extended by the latent luminosity of the snow. The blueness of the pack in this twilight, separated by the ebony lanes of open water and decorated by the algæ-strewn yellow and green lines in the hummocks, makes the scenes curiously attractive. Added to this we have the bergs, tall, sharp and imposing, standing out against the soft blue of the sky and the hard blue of the pack as if cut from huge masses of alabaster. The

whole scene is one of lively contrasts, pleasing to the eye and stimulating to the mind, having quite the reverse of the effect of the days of darkness and depressing storms which have preceded."

At about ten o'clock we saw an aurora. It began as a ragged arc, spread easterly and westerly across the southern sky, with a straight line running under it close to the horizon. The space under the arc was noticeably darker than the surrounding sky, and in this space, also a straight line, were four luminous spots. The color of the aurora was a bright cream with an occasional suggestion of pink.



EARLY EXHIBIT, MARCH 20, 1898. ARC WITH RAYS CONVERGING TO A COMMON CENTER.

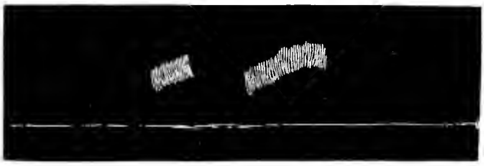
MARCH 23. FRAGMENTS OF MULTIPLE ARCS.

MARCH 24. LUMINOUS GLOW.

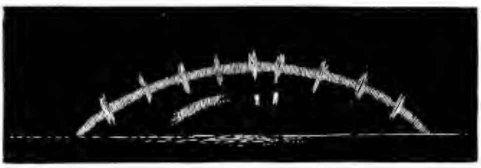
There was no noticeable reflection of light on the snow. A quick and constant transformation took place in the form of the phenomenon. A wave of light ran through the luminous bands and spots from east to west. Some parts brightened and enlarged, others darkened and faded away. The arcs were generally of a steady rayless brightness; the apparent movement and wavy effect of light were in a series of sharp rays on a film-like display before the arc.

I found it difficult in the low temperature to remain outside for periods sufficiently prolonged to catch the minute changes in force

and character, but I made a series of eight sketches at intervals of about twenty minutes apart, which illustrate the most striking changes. The second form was a homogeneous arc with a fragment of a second arc under it. This hung for some time, with a steady nebulous glow between it and the one previous, as well as between the intervening periods of all. The following typical forms then were rapid and almost imperceptible gradations. The third sketch represents the same position on the heavens; but under it are portions of two other arcs and a suggestion of a luminous horizontal line. At times a wave of rays, converging to the pole of the circle described, ran over the main arc. In the fourth sketch there are two arcs and a portion of a third which were seen persistently in all the exhibits to be present. In the fifth there is a second arc crossing the first. This was suggested by the



MARCH 26. EARLY DISPLAY. THIS WAS FOLLOWED BY ,DARTING RAYS AND WAVY RIBBONS WHICH ENDED IN A BRILLIANT ARC WITH MOVING RAYS DRAWN OVER IT CONVERGING TO A COMMON CENTER, AS SHOWN BELOW.



MARCH 26. LATER.

third, and it reappeared in the seventh. The sixth form was an arc with three ribbons of luminous beams waving from side to side. The exhibit ended with a plain arc aglow with a steady light.

For a week following we had faint auroral displays every night, but we seldom saw a brilliant or extensive exhibit. The usual form it took at this time was that of a fragment of one or several arches. On the night of the 26th we saw the usual auroral patches in the southeast which we had seen so often before. These disappeared entirely at ten o'clock, but reappeared shortly after in a manner and vividness worthy of note. There was a steady luminous bow somewhat brighter than the Magellanic clouds, and over this there were bunches of brighter rays with a rapid motion from east to west. These rays centered to a point below the horizon. Under this main arc there was from time to time a suggestion of a second and also a continuation of the same

rays which played over the main arc; above there were also occasional fragments of an arc and a prolongation of horizontal rays. This display continued until about three o'clock in the morning.

The color of this aurora, as of all those which preceded and followed, with but one exception, was a faint flesh color edged with a pale greenish-yellow. We saw no prismatic colors. The exception was a fragment of an arc in the southeast early in the evening of April 10. This was for a few moments noticeably green, but it quickly faded and vanished. Later in the evening it reappeared in the same form and place, but the color was nearly white.

In the latter part of April we saw a few auroras, especially after storms, on clear nights, but instead of increasing in number and in brilliancy, which we expected, as the veil of winter darkness was spread over us, they diminished steadily as the long night advanced. On May 17 we saw the autumnal sun for the last time. Its cold, distorted and seemingly wrinkled face lingered for a few moments on the northern ice and then sank into the frozen sea, from which it did not ascend for about seventy days. It is curious that we must say about seventy days, but this uncertainty is due to the fact that for several days before sunset the sky was obscured by storm clouds, and our constant drift with the pack-ice made our latitude uncertain.

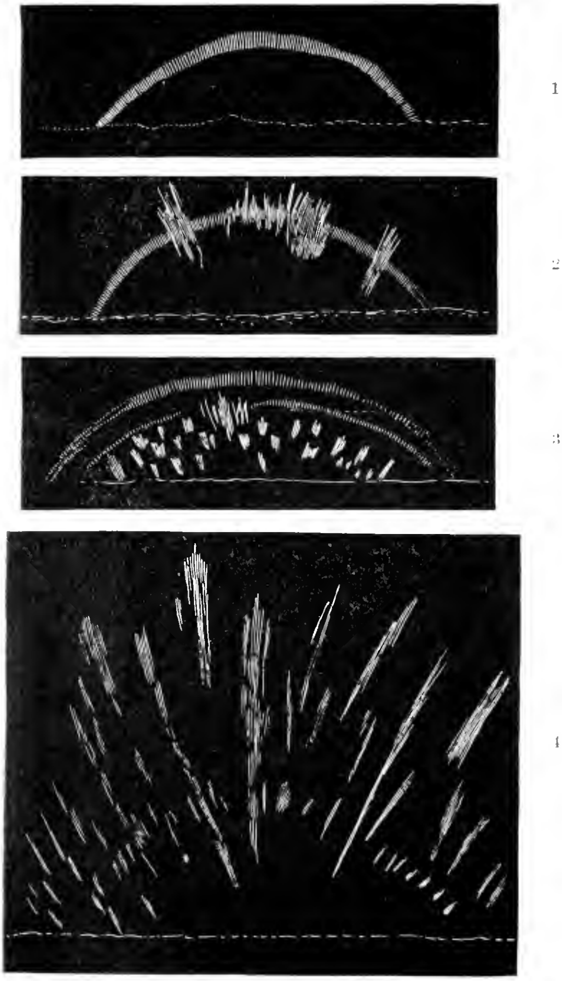
During this long night auroras were but rarely seen, but the weather was clearer and steadier than before and after. On May 21 and 22 there were faint auroral bands in the south, on the 20th there was a feeble arc in the southeast, and on the 29th there was a feeble double arc. On the 22d, 23d and 24th of June there was a similar phenomenon in the same position, and this curiously enough reappeared one month later, in July, on the same dates. The long antarctic night, then, as experienced by the observers of the 'Belgica' was not apparently lighted by the Aurora Australis.

During August we saw but one bright display, which was a double arc, on the 20th, for most of the month was so stormy that the clear sky was seldom visible. The last week in August, however, was a remarkable period of clear weather. Bright sunlight, charming moonlight and fascinating halos were among our delights in these life-giving days of the south polar spring. The sea of ice was made doubly interesting by the increasing number of penguins and seals, crying and grunting and making manifest in various ways the contentment and satisfaction of the new sunny splendor of their usually cold and cheerless abodes. From the 'Belgica' the budding passions of a new life were bursting forth; songs and laughter and a noisy commotion were audible and visible during the evening hours.

The moon often so illuminated the skies that it was difficult to distinguish between ordinary cirrus clouds and bands of auroras. On

the evening of September 2, however, there was an exhibit which could not be mistaken. I give it as written down at the time.

"Low down on the southern sky there stands a faint arc of light, and under it there is a distinct segment, darker than the sky above. This segment has been noticed in several previous auroras, but it was

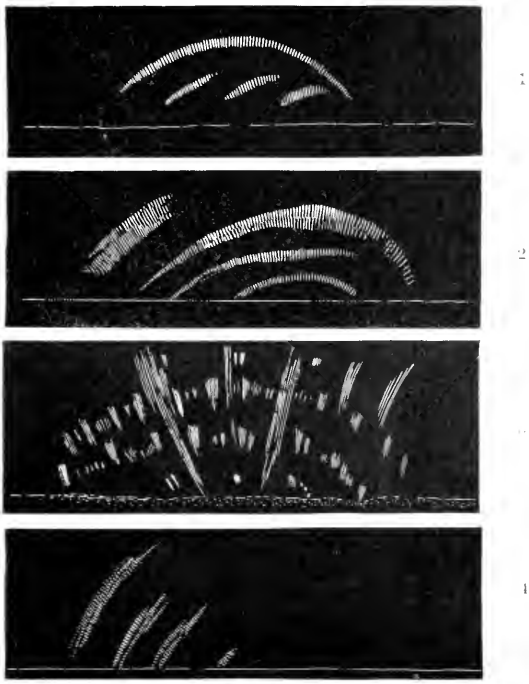


Successive Displays of an Austral Aurora on the Evening of September 2, 1898.

not before so clearly defined. On board there is considerable difference of opinion about this segment. Some have previously doubted its existence, but to-night it is indisputable. I have taken the ground that it is produced by the haze of ice crystals which always rests over the ice, and I believe that its darkness depends upon the amount of humidity or the thickness of the suspended icy haze. The stars shine

through this dark segment, apparently as bright as those above, but the light is changed in color and there is frequently a kind of halo about them. The arc gradually grows in intensity and in breadth, and it also rises a little towards the zenith. The upper edge of the segment pales as its height increases. The arc has remained perfectly regular; its two ends almost touch the horizon, and they advance to the east and to the west, widening the distance between them and showing more and more the contour of a circle as the bow of light rises."

For the first hour no beams were discernible, but the whole display consisted of an almost uniform light of a delightfully soft, cream color.

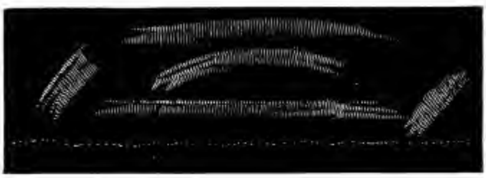


Successive Displays of an Austral Aurora. Evening of September 10, 1898.

At ten o'clock this arc was about 15° above the sea; it was about thrice the breadth of an ordinary rainbow, and its edges were clearly defined against the dark blue of the heavens. Up to this time an air of restfulness and repose was about the phenomenon, but now this began to change to an atmosphere of mysterious excitement. A wave of light rolled slowly from one side to the other. This wave soon took on the texture of torn lacework and was drawn to and fro, while the arc, which was less brilliant, remained as it had been before. At about eleven o'clock a second arc, somewhat narrower and less brilliant, appeared below the first. The play of drapery now vanished, but in

the dark segment there appeared many glowing elongated spots all pointing toward a common circle below the horizon. These came and went with such marvelous swiftness that it was difficult to follow their forms with the eye. Still later, all signs of the aurora disappeared, except the primary arc, which had for a time at its lower edge a faint suggestion of prismatic colors. This rested motionless on the midnight heavens until about two o'clock, when it slowly faded, but before it disappeared it was replaced by a bewildering display of a rayed arc.

From September 1 to the 9th the temperature steadily fell. On the 8th the thermometer registered — 13.1 C. This was the coldest spell of the year, and it was followed on the 9th and 10th by the most vivid and impressive auroral displays that we saw. The exhibit of the evening of the 10th began in quite the usual way, with a cloudless brightness in the south. Soon there appeared an arc with its ends about 10° above the ice. Under this arc there appeared three fragmental arcs. In the course of an hour the first arc nearly disappeared, leaving only a crescent strip, but under it there were bows more or less elliptical. These vied with each other, alternately brightening and fading, and



Horizontal Streamers and Parts of Multiple Arcs. Evening of September 20, 1898.

vanishing altogether or in parts, until after midnight. At about one o'clock they disappeared suddenly and in their place came, with an electric glow and swiftness, a bewitching array of ragged patches describing four arcs. One hour later these spread over the entire heavens, making a system of quivering, moving streamers, sweeping the skies and illuminating the snows with an effect perfectly bewildering.

This was the last great aurora that we saw, and it was followed by only one other, on September 20. The night at this time was so bright that the phenomenon was barely visible, but its form was different from any which we had seen. There were two horizontal bands; between these was an imperfect arc, and on both sides were crescent-shaped patches. The whole display came and went within an hour.

It is a curious fact that the auroras usually appeared about the 20th day of the month. During the long polar night, when the boreal display is at its best, we saw very few exhibits. The phenomenon had little or no effect upon the compass, but it seemed to have some connection with the storms, for it was invariably either preceded or succeeded by violent atmospheric agitation. We did not hear any sounds,

nor could we at any time perceive an odor, both of which have been reported in connection with the northern lights.

A phenomenon which displays its glories in skies so remote and under conditions so mysterious cannot fail to excite popular interest. This interest generally suggests the question, 'What is the aurora?' This was the inquiry of ancient, as it is still the query of modern students, and our answer is the repetition of the old question, 'What is it?' We are still far from a solution of the problem. We have, however, advanced far enough to put aside many old theories, and we hope that we are now on the train of inquiries which will eventually solve the mystery.

The similarity of auroral light to that generated in a vacuum bulb by the passage of electricity, it is now believed, lends support to the proposition, suggested long ago, that the aurora is of electrical origin. It was expected that that great mystery-solver, the spectroscope, would help us in this matter, but it has only served to add further mystery to the subject. For the line which it sifts from the aurora is not matched by any other known substance. A similar line is found in the zodiac light, but this does not elucidate the matter. The zodiac light, though better studied, is still as mysterious as the aurora. When electricity passes through rarefied air it exhibits a luminous stream which seems to have the characteristics of the aurora, hence it is quite probable that this natural phenomenon is the result of currents of electricity passing through the upper regions of the atmosphere, particularly by an exchange of celestial and terrestrial electricity.

The question is, however, one of the problems of the future. Though many exploring expeditions have penetrated the icy polar solitudes, very few men have given the time and patience necessary for a systematic study of the aurora. No travelers should enter the realms of the polar lights without being prepared to record, with the accuracy required by science, the plays of this heavenly mystery. I believe that, when men shall have penetrated a little farther southward into the unexplored area about the south pole, the aurora australis will be found just as brilliant, as varied and as frequent as the aurora borealis. With our present marvelous strides in uncovering the accumulating mysteries of past centuries, we ought, ere long, to look with pride and understanding into the dark vault of the polar night and read the flaming letters which must there reveal a thrilling tale of Nature's most cherished secrets.

PROGRESS AND TENDENCY OF MECHANICAL ENGINEERING IN THE NINETEENTH CENTURY.*

BY ROBERT H. THURSTON, LL. D., DR. ENG'G,
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THE progress and tendency of mechanical engineering in the nineteenth century comprehends the progress and the tendency of almost all that has distinguished the nineteenth century from all the centuries of time, historic and prehistoric, that have preceded.

The progress of the human race includes advancement in all the languages, all the literatures, all the arts and all the sciences of all times. But the progress of past time in language is the evolution of the employment of the tongue in the conveyance of ideas, and it is the idea that is important, rather than the language. Progress in literature is the perfection of our methods of permanent preservation of ideas, and, again, it is the ideas, not the systems of preservation, that count. Progress in the sciences, in a proper acceptation of the term, is the progress of the race in knowledge of the laws of nature and the phenomena of nature, the progress of reduction of such exact knowledge to system, the construction of a code of natural law in all departments of science. Progress in the arts is advancement in the utilization of Nature's laws in the construction of a system of application of the materials and forces of nature to the enrichment and elevation of human life from its crudest and simplest forms to the highest and noblest phases of civilization. Progress in mechanical engineering is the evolution of the methods and machinery of production, transportation and utilization of the material forms of wealth. In all other directions, the progress of the world has been more or less steady, continuous and evolutionary from the beginning of the life of the race; in the sciences and the arts, it has been an evolution mainly of the later times, though having an origin prehistoric.

Progress in mechanical engineering, the production of permanent wealth in most part, could only come after language should have supplied a satisfactory vehicle for ideas; it could only begin after literature should be competent to furnish a means of storage of ideas and of knowledge in safe and accessible treasuries; it could only progress rapidly after science had accumulated sufficient store of knowledge of facts, of phenomena and of natural law to permit complete reliance upon

* An address delivered before the Washington Academy of Sciences, Columbian University, February 19, 1901.

that stock of substantial learning in the effort to develop the resources of nature for use; and such advances could only go on, unimpeded, after the nature of the great sources of power in the world should have been discovered and their availability for the purposes of the engineer recognized. Mechanical engineering, as we understand it, could only fairly start in its wonderful progress after the mechanic had found ways of utilizing natural forces and energies, and of making the tools with which to produce these prime motors, through the operation of which all the arts could be given application in the production of wealth, multiplying the power of the unaided hand by making it in the performance of work the guide of greater powers, rather than the tool itself. It was only when mighty powers could be thus developed and guided and directed that mighty tasks could be performed by so weak and insignificant an organism as man. Man as a prime mover is feeble and helpless before the great powers of nature; man as the master and guide of nature's powers is only less than omnipotent.

Mechanical engineering, to achieve its highest tasks, must have control of the grandest powers of nature and of all her energies; it must avail itself of prime movers transforming all actual and potential energies into available, transformable, useful work; it must be capable of making for itself tools and machines and apparatus, scientific and other, competent to direct those energies in definite and helpful ways to the performance of every useful task. Progress must wait for the power and power must be guided, divided, applied, through invention and the mechanic arts, to defined and precisely related productive operations. The natural order is: first, sources of available energies; second, prime movers applying while developing those energies; third, tools and machines devised and constructed to perform detailed tasks, exactly and perfectly. Invention is the first necessity, and necessity has been found to be the mother of invention; but invention is helpless without tools, and invention began with the first crude tools; the motors followed, and better tools followed motors, and better motors followed the invention of better tools. It was only a century ago, or a little more, when the inventor had reached a certain stage in the production of tools, that Watt could produce the steam engine of the nineteenth century, that a system of manufactures could come into being as the fruit of invention and that the Golden Age of the centuries could begin.

The Golden Age of the World, in all good senses, had its origin with the birth of the nineteenth century, and when mechanical engineering began uniting all the sciences and all the arts into one great system of adaptation of nature's powers to the work of the promotion of civilization. This fairly begun, the steam-engine, the gas-engine, the electric motors and generators, telegraphs and telephones, the steamboat, the locomotive, the automobile, textile manufactures, iron and steel

making, shortened working hours for the people came and leisure for the enjoyment of the best that life affords, for thought, for contrivance, for self-communing, for self-exaltation in spiritual realms, became a birthright with mankind.

Mechanical engineering is the highest illustration of applied science; and applied science is the fruition of pure science. A fundamental basis for mechanical engineering could not be secured until physical science could find safe development, and this could only be when, the age of martyrdom past and perfect freedom of thought and research assured, later Brahes and Galileos could work in peace and Gilbert and Lavoisier and Faraday and Davy and their successors could devote themselves to their labors in all the fields of science without molestation. Invention could not freely develop the arts until after these master-minds had assured freedom to the more modest but none the less glorious workers in science and the arts, and to the mechanics and the inventors, and had secured that political and social freedom to work in any and all fields, irrespective of birth and caste and creed, which has only, even now, been witnessed in America. When absolute liberty of mind and body and freedom of choice of vocation had become possible without dictation by church or state or convention; when any man could pursue research and publish results, and could follow any art and could give vent to his highest and best aspirations and impulses—only then could steady progress become possible.

It was only in the nineteenth century that a Darwin could safely pronounce his judgment, that a Spencer could formulate a system of philosophy, that a Huxley could declare his conviction and a Haeckel could face the dogmatist and that warfare between science and dogma could no longer effectively repress honest work and sincere conviction. It was only in the century lately closed that the mechanic could take up any trade, without regard to the caste of his fathers, that progress became easy for the scientific Papin, the clerical Cartwright and the physicist Black, or that the instrument-maker Watt could proceed with the evolution of the heat-engines. Only in the nineteenth century came it to pass that the inventor, impelled into any line of study and experiment and labor, could unite with every other man in the same field to perfect the machinery of the world, and that invention could build up nations like Great Britain and the United States, and give wealth to Germany and to France. Only in the century just closed did it become possible to gain freedom in the competition of brain with brain for all men.

The political freedom of the United States and its admirable policy and its practice of encouraging invention by wise patent laws have now made our country the leader among nations in all fundamental modern industries.

At the opening of the twentieth century we have far better occasion than had at any time the great cynic, Carlyle, to exclaim:

"The Present Time, youngest-born of eternity, child and heir of all the Past Times, with their good and evil, and parent of all the Future, is ever a 'New Era' to the thinking man; and comes with new questions and significance; however commonplace it looks: to know *it* and what it bids us do is ever the sum of knowledge for all of us. This new Day, sent us out of Heaven, this also has its heavenly omens—amid the bustling trivialities and loud empty noises, its silent monitions; which, if we can not read and obey, it will not be well with us! . . . . But, in the days that are now passing over us, even fools are arrested to ask the meaning of them; few of the generations of men have seen more impressive days. . . . There must be a new world if there is to be any world at all! . . . One thing I do know," he adds, . . . "That the few Wise will have, by one method or another, to take command of the innumerable foolish: that they must be got to take it; and that, in fact, since Wisdom, which means also Valor and heroic Nobleness, is alone strong in this world, and one wise man is stronger than all unwise, they can be got."

How shall the wise men and the wisest men accomplish their tasks? I take it that Carlyle was also right when he prescribed the two great tasks lying before us:

"Huge-looming through the dim tumult of the always incommensurable Present Time, outlines of two tasks disclose themselves: the grand Industrial of conquering some half or more of this Terraqueous Planet, for the use of man; then, secondly, the grand constitutional task of sharing, in some pacific, endurable manner, the fruit of said conquest and showing all people how it might be done."

"Moreover," he goes on, "there are spiritual budding-times, and then also there are physical appointed to Nations.

"Thus, in the middle of that poor calumniated Eighteenth Century, see once more! Long Winter again past, the dead-seeming tree proves to be living, to have been always living, after motionless times, every bough shoots forth, on the sudden, very strangely—it now turns out that this favored England was not only to have had her Shakespeares, Bacons, Sydneys, but to have had her Watts, Arkwrights, Brindleys! We honor greatness in all kinds. . . . Prospero can send his Fire-demons panting across all oceans; shooting with the speed of meteors, on cunning highways, from end to end of all kingdoms; and make Iron his missionary, preaching *its* evangel to the brute Primeval Powers, which listen and obey. . . . Advancing always, through all centuries, in the middle of the eighteenth they *arrived*. The Saxon kindred burst forth into cotton-spinning, cloth-dropping, iron-forging, steam-engining, railwaying, commercing and careering towards all the winds of Heaven."

Carlyle saw more clearly than perhaps any other man of his time that, as others have since said, the world owes absolutely nothing, in its conquest of the forces and powers of nature, to the kings and princes or to the aristocracy of the worlds, past or present; they, with their battles and contentions and their subordination to their own insignificant affairs of every element of real progress, have been the great impediments to progress. The world owes all rather to the inventor, to the mechanic, to the man of science and the man of mind. All progress has been effected irrespective of, if not in spite of, the acts and famous deeds of

kings and warriors, and through the arts of times of peace, or through revolutions which have been effective protests against the infringement of liberty and the restriction of the worker. Science, applied science, invention and the industrial army have done the work.

With the opening of the twentieth century we are indeed arrived at a Day of Great Things, the fruition of all those forces and movements and evolutions which have been the characteristic features of the history of the nineteenth century. All great works are performed on a mighty scale, and the advances of the industrial army are now made through wide-spread and far-reaching movements of army corps, instead of, as but two or three generations ago, in a thin and straggling line of individual skirmishers. All the world is falling into line, and the whole world-wide army is moving in concert if not under a single generalship. In the industries, the captains of industry, once commanding squads and companies, now are become majors with their battalions, colonels with their regiments, generals with their brigades, their army corps, with mighty armies overspreading all the fields of production of a whole country, even of many countries. Where the single worker labored hour by hour through the long day, 'from sun to sun,' in the days of our grandparents, companies of workers now cooperate, by subdivided and wonderfully trained tactile talent, in a single multiplex task; the squad of workers in the little factory or mill has grown into an organized body numbering regiments. A whole industry is organized and supplies an enormously expanding market with continually improving product, at steadily declining costs and prices, while, at the same time, giving its armies of workmen and workwomen steadier work, at better wages, under more reasonable and comfortable conditions, day by day and year by year. The higher the wages paid and the shorter the working hours, the less the cost and the lower the price of the product, the greater is the purchasing power of the day's work and of the dollar paid the worker. This is the nineteenth century statement of the Law of Supply and Demand.*

Goethe, poet, man of science and seer, prophesied that the nineteenth century would solve the problems of organization of the industries and the great social and economic problems of an industrial epoch. Carlyle saw the same problems in progress of solution, and his disquisition on the organization of labor as the problem of the coming days shows that great men here thought alike. Hitze defines the standing problem of our time, the problem of the nineteenth century, particularly—already partly, yet not wholly, solved—to be that of finding a social organization corresponding to the modern conditions of produc-

* The Modern Version of the Law of Supply and Demand.—R. H. T.—Science, 1898.

tion; just as the social organization of the Middle Ages was adapted to the simple industrial conditions of that time.* Henry Dyer's 'Evolution of Industry'† traces this process of solution of these problems, so far as solved to date, in a most interesting way. His conclusion, that the mechanical development of the past century is a necessary element of the evolution of society, as well as of the industries, is as sound as is his deduction that the problems of the twentieth century should be solved in such manner as to insure a final evolution of an ideal, discreet, wise, prudent, pleasant and righteous life, which shall conform to the ideals of the scholar, the gentleman, the seer and the poet. On the organization of the mechanical industries largely depends the future of the world, and in this evolution of a finer and better life, through industrial and social evolution, the influence of one such man as Dolge, at Little Falls, N. Y., and of one such firm as the famous Patterson's at Dayton, Ohio, tells more powerfully than all polemic discussion.

Thus the organization of the workshop and the humanizing of the workman, as Ashbee denominates it, may be expected to proceed together.‡

The noble view of the Bishop of Durham, as expressed a few years ago, may well be taken as the enunciation of the problem and the purpose of the coming centuries:§

"Manufactures, trade, commerce, agriculture, if once the thought of personal gain can be subordinated to the thought of public service, offer scope for the most chivalrous and enterprising and courageous. It can only be through some misapprehension that it seems nobler to lead a regiment to the battlefield than to inspire the workers in a factory with the enthusiasm of labor."

He anticipates, nevertheless, that the time is coming, surely if slowly, but possibly quickly, when the Great Industry will be "made to contribute to the material and moral elevation of all who are engaged in it, not as separate or conflicting units, but as parts of the social organism."

In his remarkable little book, 'Our Country,' Dr. Strong, fifteen years before its close, affirmed that the later years of the nineteenth century constitute a 'focal point' in history, and are second only in importance to "that which always must remain first, viz., the birth of Christ." He goes on to say in his introduction:

"Many are not aware that we live in extraordinary times. Few suppose that these years of peaceful prosperity, in which we are quietly developing a continent, are the pivot on which is turning the nation's future. And fewer still

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* 'Die Quintessenz der Socialen Fragen.'

† 'The Evolution of Industry'; By Henry Dyer, C.E., M.A., D.Sc., New York and London, Macmillan & Co., 1895.

‡ 'Workshop Reconstruction and Citizenship.'

§ Economic Review, October, 1894.

imagine that the destinies of mankind, for centuries to come, can be seriously affected, much less determined, by the men of this generation in the United States."

The nineteenth century has been the first distinctively machine-using period. Until heat-motors could be found, it was impracticable to employ machinery in any very great extent in the performance of the work of the world. Until entire freedom of the worker could be assured, enabling him to devise and to find means of constructing machinery, inventions could not find general use. Until the machines for machine-making could be had, the general use of machinery could not be secured, simply because the finer classes of machinery could not be accurately and satisfactorily made. Until the modern system of manufacturing and of working to gauge, and of interchangeable parts, could be adopted, the production of an industrial system for mechanical production was not practicable. Thus all kinds of mechanisms and every department of invention waited upon every other until, nearing the beginning of the nineteenth century, the long-delayed conjunction was attained, the beginning of the machine-using age introduced a new era, and the world took a sudden leap forward; thenceforward advancing with a continuous acceleration. Then came a machine-using world. Then one man became equal, in productive power, to two or five, or sometimes to ten, or even to a hundred, lacking the aid of the machine. For the first time in the history of mankind, a real, permanent, rapid and rapidly increasing progress began. One man then became able to do the work of four of his predecessors in making agricultural machinery, and he made it incomparably better; one man could do the work of fifty in making gun-stocks, after the Blanchard lathe for turning irregular forms had been adapted to the task of aiding him. One man does the work of six in boot and shoe making; in some departments of textile manufacture one man with his machinery does the work of a hundred of earlier generations. In fact, the earlier generations from prehistoric days have no record of any very important progress. Each man, with his modern newspaper-printing press, taking its paper from its miles of rolls, printing, cutting into sheets, pasting together, folding and piling ready for the carrier, does the work that five hundred men would have been employed to do a century earlier, and then it would have been a work very badly done, as gauged by our standards. Mr. Wright reckons that the machinery of the United States gives the power of doing work that, without it, would require the labor of a hundred millions of workers; thus multiplying the average work of the average individual worker by about six.* In a very large proportion of the later developments, especially in the application of steam-power,

* 'Industrial History of the United States'; Chautauqua-Century Press, 1895.

the task of the machine could not be performed at all; as, for example, the haulage of the railway train at the rate of a mile a minute or more, or the driving of a transoceanic liner across three thousand miles of stormy seas at the speed of fifteen to twenty-five miles an hour. A large fleet would be required to carry the labor and provisions for a single craft, equivalent in total power, driven by animal force.

It has been the use of machinery that has permitted the people of our country to use twenty pounds of cotton *per capita* where they formerly used but five; to increase their consumption of iron and steel from a few pounds to four hundred *per capita*, and the consumption of steel from an insignificant amount up to two hundred and fifty pounds and over. This use of machinery and universal extension of mechanical engineering has even permitted the doubling of our population in the last generation and the increase in the number of people employed in productive vocations one hundred and seventy-five per cent., according to Mr. Wright's statistics. One-sixteenth of the whole population of the country is supported by labor in railway transportation, and the once minute proportion engaged in coach and other transportation by means of horses has been multiplied many times over; yet these people were the most vigorous and powerful of all opponents of this advance.

The sewing machine, giving the power of multiplying many times the productivity of the needle, instead of displacing the sewing girl, as once anticipated by many ignorant persons, proves to be not properly a labor-saving machine, but a machine assisting labor and multiplying not only the efficiency of the worker, but also many times increasing the quantity of work to be done and the numbers needed for the work, as occurs in all such cases. In a thousand instances the invention of a machine or the introduction of a new method in some department of mechanical engineering has brought into use an entirely new form of industry and made effective and productive the highest powers of thousands of people who otherwise would probably be compelled to drudge on in old ways and to content themselves with old rates of compensation. The reduction of costs of product and the increase in the wages of labor are the two most striking results of the revolution of the century; while the accession of value conferred upon material is sometimes quite as impressive, if not actually as important—as when the often-recounted fact is noted of the increase of the value of iron from one dollar in the ore to five or six in the shape of iron bar, to $10 in any one of many finished machine-made forms, to $200 and upward in fine cutlery, to $5,000 and $10,000 in needles, to $200,000 and more in common watch-springs, and to perhaps half a million dollars in the shape of hair-springs, the most refined condition of the metal yet attained, or even up to $2,500,000, according to Mr. Woods, in the form of pallet-arbors.

The earlier days of the factory system, while an improvement upon what had preceded, were not particularly promising, as viewed from this latter-day standpoint. The manners and morals of the time were necessarily imported into the factory, and, while the working men and women and children soon gained a higher plane of comfort, that level would seem to us an exceedingly small advance upon the conditions of the Middle Ages. The hours were long, the wages low, the social conditions in all respects still unsatisfactory. Women and children were flogged for actual or imputed idleness, for incompetence, or even for lack of endurance and of strength. But the output soon filled the markets of a then poor and moneyless people, and wages and prices began to readjust themselves, as always, to the new commercial conditions, and the end of the century has found the operatives organized and powerful, knowing and compelling fair treatment, sometimes even tyrannizing over the employer, indeed, but usually simply securing by force what is fairly theirs and can not be otherwise obtained. Wages have risen and prices fallen, until the day's work earns several times as much as a century ago. Moral tone has improved as social conditions have thus been improved, and comfortable houses, plenty of good food and many of the luxuries of the beginning of the century are taken as ordinary comforts by the operatives of to-day. Then the wealth of the country was $320 *per capita*, to-day $1,350; then it aggregated seven and a half millions, to-day its total is about a hundred billions, increasing thirteen times, while the *per capita* account has risen to four times its initial figure. In a half-century these workers have increased their *per capita* account as depositors in the savings banks from about $175 to about $400; the total deposits growing from something over forty millions to about twenty-five hundred millions. The factory system is also the source of all labor legislation, and this, on the whole, splendidly beneficent code is thus due to the perfection of the methods of mechanical engineering. Society is advantaged from top to bottom and in every class, most of all in the humblest.

While it is true, as I have sometimes asserted, that "great movements, whether of mind or matter, of nations or of plants, of civilization or of comets, have definable rate and path," and while it is the fact that, as I have put it, 'Nature turns no sharp corners' in such mighty fluxes of energies, it is nevertheless true that some of her movements have paths of considerable deviation from the right line, and some of the motions illustrated are occasionally almost as tremendous accelerations as is the irruption of the volcano after its long period of preliminary development and storage of energy. The process of evolution is continuous; but its action involves every variety of acceleration and visible change, even though the evolution is itself a steady operation. We see only portions of its action and lose sight of its continuity and

its steadiness. So it has happened that the nineteenth century has illustrated such an apparent, though not real, exception to the law. The forces of civilization had been cumulative; the resultant forces gathering through the earlier ages, partly stored and latent, but none the less potential, and partly as the actual and kinetic energies of phenomena of visible evolution. All energies seem to have become kinetic and visible in their aggregate results in this Victorian Era. The outcome has been described as a 'tidal wave' of upward and onward movement on the sea of universal progress, a climax of an evolution of which the earlier periods have been quiet and silent and simply those of preparation. It has been like the action of the seas beating upon a yielding shore. Slowly and steadily through the ages it cuts farther and farther into the obstruction, unobserved and unrealized as a great natural movement, until, at last, the dike is cut through and the ocean rushes in and overflows the land. This flood, beneficent as the other might be destructive, has had a somewhat similar history. The nineteenth century is the period of uprush and inrush of the flood of efficiently applied human intellect, making effective al those powers which have been till now frittered away, the magnificent potentialities of which have never been before realized.

This volcanic development of previously latent, but gathering and cumulative, energy has been effective in every department of human activity, but most of all, perhaps, in the field of invention, of the mechanic arts, of what we have come to-day to designate 'mechanical engineering.' The acceleration has been one beside which that of the falling stone or a dropping shot or the meteor precipitated into the field of attraction of our globe seems insignificant in resultant effects. In the year 1800 we had not a locomotive or a railroad for public service in the world. To-day the United States alone, with half the mileage of the world, possess 200,000 miles, nearly, of rail and about 40,000 locomotives. Then we had no telegraph; to-day its wires span the continents and carry messages along the bed of every ocean, binding the continents as with ties of steel. Over three millions of miles of wire transmit three hundred to four hundred millions of messages annually, and nations are brought within speaking distance and bound heart to heart. The events of the antipodes are signalled to us, hour by hour, as they occur, and we read at the breakfast table of battles, coronations, deaths and births of individuals and of nations, of all the great phenomena of a world, from Atlantic to Pacific and to Atlantic again, and almost from pole to pole.

(To be concluded.)

PRIMITIVE COLOR VISION.*

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THE importance of language as an instrument of anthropological enquiry has been the subject of much difference of opinion. On the one hand, there are those who believe that the relation between language and thought is so close that the former has always been an almost exact mirror of the latter, and that every increase in intellectual development has been accompanied by, if not conditioned by, a corresponding increase in the development of language. On the other hand, the tendency which perhaps now prevails among anthropologists is to attach too little importance to language as an indication of the mental development of a race. The subject of the color sense of primitive races is one which is especially useful in studying how far the capacity for appreciating differences goes with the power of expressing those differences in language. We are able to put to the test how far the ideas of a people may be deduced from their language. We can collect the epithets used for color in various races, both of the present and of the past, and from a study of these epithets we can draw conclusions as to the nature of the color sense in these races. In the case of still existing races, we can then examine the color sense objectively and ascertain how far the conclusions derived from the study of language are verified by the result of the objective examination.

Historically, this is more or less what has been done. In 1858, in his 'Studies on Homer and the Homeric Age,' Gladstone called attention to the great vagueness of the color terminology of Homer; he showed that Homer used terms for color which indicated that his ideas of color must have been different from our own, and he was inclined to go as far as to suppose that Homer had no idea of color as we understand it, but distinguished little beyond differences of lightness and darkness.

Ten years later, Geiger,† from a more extended investigation of ancient writings, also came to the conclusion that the color sense of the ancients must have been very defective. He found, not only in Greek literature, but in the Vedic hymns of India, in the Zendavesta, in the Norse Edda, and in ancient Chinese and Semitic writings that there was evidence of great imperfection, especially in the names for green and blue. In hardly any of these ancient writings is any word used from

* A lecture delivered at the Royal Institution, January 25, 1900.

† 'Contributions to the History of the Development of the Human Race,' p. 48.

which we may gather that the people who wrote them had any idea of blue. Geiger advanced the view that there had been an evolution of the color sense in historical times; and he supposed that this evolution had been of such a kind that red had been distinguished first, followed by yellow and green, and that the sense for blue had developed much later than that for the other colors. Magnus* came to the same conclusions on the basis of a still more extended examination of ancient writings, and Gladstone, in 1877, again called the attention of English scholars to the subject in the pages of the 'Nineteenth Century.' †

The subject was taken up both from the literary and scientific points of view. On the literary side it was objected that the special peculiarities of the color terminology of Homer were due to a characteristic of epic style, according to which attention is paid to form rather than to color. It was also pointed out that the language of some modern poets presents the same peculiarities as those of ancient literature. Grant Allen ‡ counted the color-epithets used in Swinburne's 'Poems and Ballads' and found that red occurred much more frequently than blue, and a similar preponderance of red was found to be a feature of Tennyson's 'Princes.' Instances were also given of individual peculiarity in the use of color by many modern poets, one instance being La Fontaine, who, according to Javal, § only used an epithet for blue once in the whole of his poems.

On the scientific side, also, objections were raised. It so happened that about 1877 there were in Germany two parties of Nubians going from town to town in traveling caravans. These Nubians were examined by Virchow and others, and it was found that they exhibited the same peculiarity of color language as ancient writers; they had no word for blue, or, rather, they used the same word for blue as for black or for dark colors generally. On examination, it was found, however, that they were not color blind, and that they sorted colored papers and wools correctly. It was, therefore, concluded that the ideas of Gladstone and Geiger were altogether erroneous, and that there was no necessary connection between color sense and color language.

In this country the views of Gladstone and Geiger were submitted to a comprehensive criticism by Grant Allen in the book, 'The Color Sense,' already cited. The strongest objection raised by this author was based on the existence of a well-developed color sense in many of the lower animals, and it was argued that this sense could not therefore be defective in primitive man. He also brought forward evidence that many existing primitive races made large use of color, and cited the

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* 'Die geschichtliche Entwickelung des Farbensinnes,' Leipzig, 1877.

† Vol. II., p. 366; 1877.

‡ 'The Color Sense.' London, 1879; p. 264.

§ 'Bull. de la Soc. d'Anthropologie de Paris.' T. XII., p. 480; 1877.

opinions of travelers that savages were able to distinguish colors perfectly. He also pointed out that the decorations of the early Egyptians and of other ancient races showed the existence of a well-developed sense long before the time of Homer.

The controversy was carried on for some years, especially in Germany. Magnus* showed that the same defect of terminology for green and blue, which characterizes ancient writings, still exists among many primitive races at the present day, and argued that this wide-spread peculiarity must have had some definite cause, probably of a physiological nature. Nevertheless, the general trend of opinion was strongly against the views of Gladstone and Geiger, and the idea of the evolution of the color sense in man has been almost universally rejected.

As a member of the anthropological expedition which went out from Cambridge to Torres Strait and New Guinea in 1898, under the leadership of Prof. A. C. Haddon, I had an opportunity of re-investigating this question. In addition to a full examination of the color vision of two tribes of Papuans inhabiting one the eastern and the other the western islands of Torres Strait, I was able to make observations on natives of the Island of Kiwai, at the mouth of the Fly River, and on members of several Australian tribes. The languages of these people showed different stages in the evolution of color terminology, which correspond in a striking manner with the course of evolution deduced by Geiger from ancient writings. In an Australian tribe, from the district of Seven Rivers, on the eastern shore of the Gulf of Carpentaria, several natives only used three color epithets; red, purple and orange were called by the same name, 'ŏti'; white, yellow and green were called 'yŏpa,' while black, blue, indigo and violet were all called 'manara.' Other natives from an adjoining tribe used the names 'owang,' 'wapŏk' and 'unma' in the same sense; some natives applied other names to green and yellow, but those given appeared to be the only terms which were used with any definiteness and constancy.

The next stage in the evolution of a color vocabulary was found in Kiwai. In the language of this island there was a very definite name for red, 'dŏgó-dŏgó,'† and a less definite name for yellow, 'agó-agó agó-agó.' Greens were called by most individuals, 'emasóro' and 'tigiro,' which were also used for white and black respectively, and may probably be translated 'light' and 'dark.' A few used a special word for green, 'poroporona.' Blue and violet were usually called 'wĭbu-wĭbuna,' the word for black, others calling these colors 'tĭgiro' or 'pŏropŏrona.' The brilliant blue of the sky received from these people the same name as the deepest black.

* 'Untersuchungen über den Farbensinn der Naturvölker, Jena, 1880,' and 'Ueber ethnologische Untersuchungen des Farbensinnes,' Breslau, 1883.

† 'Ó' stands for the sound of 'aw' in the word 'law.'

In Murray Island, in the eastern part of Torres Strait, where we stayed for four months, I was able to investigate the language used for color very completely. In this island there was a very definite name for red, 'mamamamam,' while several other names, such as 'kiami-kiam,' 'erŏko, mamamamam' and 'somer-mamamamam,' were used for purples and pinks. There were two definite names used for both orange and yellow, 'bambam' and 'siusiu,' and one definite word for green, 'soskĕpusoskĕp,' while several other words were occasionally used. There was, however, no native name for blue, apart from that used for black, 'golegole.' This word was used by many of the older men, and, as in Kiwai, the brilliant blue of the sky and the deep blue of the sea would often be called by the same name as the darkest black. Many of the natives had, however, adopted the English word, which, by re-duplication and separation of contiguous consonants, had become 'bŭlu-bŭlu,' and many of the younger men believed that this word belonged to their own language.

The color language of the western tribe of Papuans in Torres Strait was fully investigated in the Island of Mabuiag. Here the vocabulary was more definite. There were a limited number of terms which were used by nearly all for the chief colors. Red and yellow were called 'kulkadgamulnga' and 'murdgamulnga' respectively. There was a fairly definite term for green, 'ildagamulnga,' which was, however, sometimes used for blue, and there was a term for blue, 'malud-gamulnga,' which was also used not infrequently for green. In addition to these four more or less definite color names, other terms were used for different shades, and a few natives showed extraordinary ingenuity in devising special names, apparently on the spur of the moment, for different shades of color. I have a list of over thirty such names from one individual, all derived from a comparison with natural objects.

In these four languages of Seven Rivers, Kiwai, Murray Island and Mabuiag, we have progressive stages in the evolution of color language; in the lowest there appears only to be a definite term for red apart from white and black; in the next stage there are definite terms for red and yellow, and an indefinite term for green; in the next stage there are definite terms for red, yellow and green, and a term for blue has been borrowed from another language; while in the highest stage there are terms for both green and blue, but these tend to be confused with one another. It is interesting to note that the order in which these four tribes are thus placed, on the ground of the development of their color language, corresponds with the order in which they would be placed on the ground of their general intellectual and cultural development.

It is said that there are other races, such as the Todas of Southern

India, who have only three definite terms in their color vocabulary, viz., those for red, white and black, while others have also a term for yellow. The absence of a definite term for blue, on the other hand, is very common. The languages which have this characteristic appear to fall into two main classes; those which, as in Kiwai, have the same word for blue and black, and those which have the same word for blue and green. The former class includes the languages of Hovas and Bushmen, as well as of many Australian and Melanesian tribes. The second group comprises a very large number of languages, including one so near home as Welsh, in which there is only one word, 'glas,' for both green and blue.

By many races a word for blue has been borrowed from some other language, as was the case in Murray Island; thus many African races are said to use the term 'bru,' obviously a corruption of the English word; in South America the Spanish word 'azul' has been borrowed, and the Battas of Sumatra have borrowed words both from Dutch and Malay. The word used by the Ga people of the Gold Coast for blue and for indigo is said to mean literally 'something that must be learnt,' these people having been taught the use of indigo either by Europeans or by other Africans.*

When in Ceylon I obtained color vocabularies from a number of Singhalese and Tamils, and, though the two languages differed in other respects, both Singhalese and Tamils used the word 'nīl' or "nīlam' for blue, and this word, which is said to be the same as the name of the river Nile, is found widely distributed among Asiatic languages. The river Nile has another interest in connection with this peculiarity of color language. We are in the habit of speaking of the White Nile and the Blue Nile. The Arabic name for blue is 'azrag,' a word used by the modern Egyptian for blue and for dark colors generally. 'El Bahr azrag' probably originally meant the dark Nile, and, when we speak of the Blue Nile, we are using an expression which is based upon the primitive confusion between blue and black.

Magnus has shown that these defects in color nomenclature cannot be referred to the poverty of language. Some races, such as the Kaffirs and Basutos of South Africa, who have no word for blue, have, nevertheless, a very long vocabulary for the various colors of oxen. Similarly, the Kirghises,† of Central Asia, have many different names

* For many other instances of defective color terminology among savage and semi-civilized races, see the papers of Magnus already quoted, and also Andree, 'Zeitsch f. Ethnologie,' Bd. X., p. 328; 1878.

† Radloff, 'Ztschr. f. Ethnol.' Bd. III., p. 285; 1871.

for the colors of horses,* though they tend to confuse the designations for green and blue.

I have had an opportunity of examining the color vision of the Eskimos who have lately been in London, and have found that their language presents a very striking contrast to that of the tropical people, to whom my previous work had been limited. The terminology for color appears to be extremely well developed; there are definite names for red, yellow, green and blue, and modifications of these colors are expressed by means of suffixes or by compounding two names; thus, by more than one individual a purple was called 'aupaluktaktungalangaijuk,'† which means bluish-red, while a violet was called 'tungajuktakaupalangaijuk,' which means reddish-blue. This recognition of the fact that violet and purple are mixtures of red and blue shows a high degree of definiteness in the nomenclature of both colors. I have only met with one other individual who behaved in a similar way, viz., a Tamil, who called purple 'sikapu-nilam,' red-blue.

Another peculiarity which appears to characterize a very large number of languages is the absence of a word for brown. In Torres Strait a native would call one brown by a name meaning 'reddish'; another brown by the same name as yellow, while others would be called dark or gray. It was quite clear that they had no generic name for brown. In the Australian and Melanesian languages, I have had the opportunity of studying, as well as in Tamil, Singhalese and Eskimo, I have failed to find any definite term for brown, and the same defect is found in Welsh and in many other languages. The word given for brown in many vocabularies is obviously the same word as that used for red or dark or gray. There is always a danger that one may accept, as a generic name for brown, a word which is only a name for a special brown. This was very well shown in Mabuiag, where brown wools were by some natives called by such names as 'wamauwibadgamulnga' (honeycomb colored), or 'wabadgamulnga (Dracæna colored), but it was quite certain that these were names used by certain individuals for special browns and were in no sense names for brown in general. Similarly, in other languages in which there is no word for brown there may be names for special browns, such as names for the colors of horses or cattle, but such terms are limited to those objects. We have in our own language similar examples in the words 'bay' and 'dun.'

The idea of brown is so definite with us that it is surprising that a word for brown, and apparently the generic idea of brown, should be

* Strictly speaking, these names and those of the Kaffirs are not names of colors, but rather names for distribution of color and marking; thus among the Kirghises a brown horse with a black mane and tail would have one name, and a brown horse with a white mane and tail, another name.

† 'Au' stands for the sound of 'ou' in 'house,' and 'ai' for that of 'i' in 'ice.'

absent from so many languages. The fact is perhaps the more strange in that the word 'brown' bears evidence of having arisen in an early stage of our language, and is not, like violet or orange, obviously of recent origin.

The special characteristics of primitive color language appear, then, to be the following: The existence of a definite name for red, sometimes with subsidiary names for shades of red; a definite name for orange and yellow; indefinite nomenclature for green; absence of a word for blue, or confusion of the terms for blue and green, and absence of a word for brown, a brown object being called red, yellow or dark, according to its prevailing character.

These features closely resemble those of Homer's color terminology, Homer uses several words for red, *φοῖνιξ*, *φοίνιος*, *μίλτος*, *ἐρυθρός* and *πορφύρεος;* he has a definite word for yellow, *ξανθός*, an indefinite word for green, *χλωρός;* and no word for blue. Two words which later came to mean blue, *γλαυκός* and *κυάνεος* were used by Homer, but it can not be said that the terms mean more than 'light' and 'dark' respectively. There seems now to be little doubt that *κύανος*, the substance, was 'lapis lazuli' and also an imitation of this substance made by coloring a glass paste with salts of copper, but *κυάνεος* is not used by Homer as an epithet for any distinctively blue object (except *κύανος*), while it is used for a perfectly black garment.* The substance, *κύανος* is also qualified in one place,† as *μέλας*.

There appears, also, to be no word for brown in Homer, but brown or brownish objects are qualified by the same adjectives which are used for red, thus *φοῖνιξ*‡ is used for the color of a horse, and *δαφοινός* is applied to jackals§ and the skin of a lion.¶

The resemblance is so striking that the conclusion seems irresistible that we have to do in Homer with a color vocabulary in the same early stage of development which is found among many primitive races at the present day. Indeed, one might almost go so far as to say that Homer's terminology for color is in a stage of development which is on much the same level as that of Kiwai, and distinctly less developed than those of Murray Island and Mabuiag.

From the nature of the defects of language, it has been concluded that the color sense of both ancient and existing primitive races is in some way defective. The next stage in the inquiry is to investigate the color sense of some existing people, and this I have been able to do most satisfactorily in Murray Island. I tested about 150 natives of this island with Holmgren's wools for color-blindness, and failed to find one case in which there was any confusion between red and green, the

* Il., XXIV., 93.

† Il., XI., 24.

‡ Il., XXIII., 454.

§ Il., XI., 474.

¶ Il., X., 23.

common form of the defect in civilized countries. Since red-green blindness exists in about 4 per cent. of the male population of Europe, one may conclude that this form of defective color sense was either absent or was much rarer than with us. I also failed to find a case of color-blindness in Kiwai and Mabuiag and among the Australian aborigines, although I met with three well-marked cases of red-green blindness among eight natives of the Island of Lifu, in the Loyalty Group.

As regards other colors, however, the case was different; blue and green were constantly confused, and also blue and violet. Either owing to lack of interest or to some actual deficiency in color sense, there was a distinct tendency to confuse those colors for which their terminology was deficient. I have also found this tendency to confuse green and blue in several other races.

The behavior of the people in giving names to colors also pointed frequently in the same direction. I have already mentioned that in Mabuiag there was a great tendency to invent names for special colors; on one occasion a man, who seemed to have a special faculty in this direction, gave me as the name for a bright blue wool 'idiiridgamulnga,' which meant the color of the water in which mangrove shoots had been washed to make 'biiu,' an article of food. In this case there was a deliberate comparison of a bright blue with dirty water, and I frequently came across other instances of the kind, which seemed almost inexplicable, if blue were not to these natives a duller and a darker color than it is to us.

This view was confirmed by quantitative observations, made with Lovibond's Tintometer, which had been kindly lent to the expedition by Mr. Lovibond. When the native looked into this apparatus he saw two square patches of light, either of which could be colored in any intensity of red, yellow or blue by means of a delicately-graded series of glasses of those colors. The 'threshold' for each color was then determined by finding the most faintly colored glass which the native could recognize and name correctly. The results showed that the natives recognized a very faint red, a more pronounced yellow, and only recognized blue when of a considerable intensity. Similar observations made on a series of Englishmen showed greatest sensitiveness to yellow and somewhat less to red and blue. The results may be given more definitely in Mr. Lovibond's units of color; in Murray Island, red was perceived on the average at .18 units, yellow at .27 and blue at .60, while the average results for the English observers were .31, .20 and .36 respectively. These figures do not show anything approaching blue blindness, but they do show a relative insensitiveness to blue in the Murray Islander, as compared with the European. The former appears also to be relatively more sensitive to red.

Another method of investigating the subject quantitatively which I employed was to determine the distance at which small spots of different colors could be recognized. I found in Murray Island that natives could see a red spot 2 mm. square at over 20 meters, while a blue spot of the same size was confused with black at even 2 or 3 meters. Europeans, however, also recognized red at a much greater distance than blue, and I have not at present sufficient comparative data to enable me to say that there is any marked difference between the Murray Islander and the European in this respect.

These results do not show that these islanders are blue blind, but they do show fairly conclusively that they have a certain degree of insensitiveness to this color, as compared with a European. We have, in fact, a case in which deficiency in color language is associated with a corresponding defect in color sense.

On the question of the cause of this insensitiveness there is room for differences of opinion. It is, of course, possible that the insensitiveness may be apparent only and may be merely due to lack of interest, but there is, I think, little doubt that it depends on physiological conditions of some kind.

The Murray Islander differs from the Englishman in two important respects; he is more primitive and he is more pigmented, and his insensitiveness to blue may either be a function of his primitiveness or of his pigmentation. In other words, it is possible that his insensitiveness may depend on the lack of development of some physiological substance or mechanism, which acts as the basis of the sensation blue in ourselves, or it may only depend on the fact that the retina of the Papuan is more strongly pigmented than that of the European. There is some reason to think that this latter factor is the more important. We know that the macula lutea in the retina, which contains the region of most distinct vision, is pigmented, and that as a consequence of the reddish-yellow color of its pigment, blue and green rays are more strongly absorbed than red and yellow; we have reason to believe further that the macula of dark races is more pigmented than that of ourselves.

The consequence would be that, in dark races, blue and green would be more strongly absorbed, and consequently there would be a certain degree of insensitiveness to these colors, as compared with red and yellow. In the observations made with the tintometer, the patches of color were so small that only the macula would have been stimulated. The probability that the insensitiveness to blue depended, at any rate partially, on the pigmentation of the macula lutea is increased by the fact that the natives were able to recognize blue readily on the peripheral retina.

It would, of course, be wrong to make any wide generalization on the basis of these observations. One would not be justified in directly

applying the conclusions arrived at in the case of Murray Island to all existing races whose color nomenclature is defective, and still less so in applying them directly to the races of the past. Nevertheless, the fact remains that, in the only race which has been investigated with any degree of completeness,* the characteristic defect in color language has been found to be associated with a corresponding defect in color sense.

There are other sources from which evidence on the evolution of the color sense in man may be derived. It has already been mentioned that at an early stage in the controversy the evidence of ancient monuments was brought forward against the views of Gladstone and Geiger. It was pointed out that, long before the time of Homer, green and blue pigments were used in Egyptian sculpture and decorations. In the Berlin Museum there is a palette with seven depressions, which appear to have been used for seven colors, white, black, red, yellow, green, a bright blue and a dark color which may have been either blue or brown. Indeed, blue appears to have been the predominant color of Egyptian pottery, and blue and green beads have been found in the graves of the prehistoric Egyptian race. Green and blue appear also to have been used in the decoration of the ancient Assyrians and Chaldeans. Greek architecture has also been found in Thera, Tiryns and Mycenæ of a date earlier than that of Homer, in which the colors used include blue.

Mr. Bénaky, of Smyrna, has recently collected† the evidence derived from the coloration of ancient monuments, and believes that it decisively disproves the existence of any defect of color vision of the ancient Egyptians and Greeks. Two considerations must, however, be borne in mind when dealing with evidence of this kind. In the first place, it may be conceded that the monuments of the Egyptians show that these people had a perfectly developed color sense, and yet the color sense of the Greeks one thousand years or more later may have been defective. Just as we find different races at the present day in different stages of evolution as regards color, so it may have been three or four thousand years ago. The state of the color sense of the Egyptians has no direct bearing on that of the Greeks. It is a point of interest that the high development of the color sense in the ancient Egyptians, as shown by their decorations, appears to have been accompanied by a corresponding development of language, for it is stated‡ that in the ancient Egyptian language there were two words for green and one for blue.

* A full account of these investigations will be given in the reports of the Cambridge Expedition.

† 'Du sens chromatique dans l'antiquité.' Paris, 1897.

‡ See 'Kosmos,' Bd. I., s. 430; 1877.

The other consideration, to which, in my opinion, sufficient attention has not been paid, is whether the various colors are used appropriately. To prove the existence of a well-developed color sense from monuments, it is not enough to show that certain colors were used; it must be shown that they were used appropriately. Even in the case of Egyptian art, one reads of statues of human figures with blue hair, and in the case of early Greek art, the inappropriate use of one color, blue appears to have been very common.

I am informed by Mr. E. E. Sikes, to whom I am glad of this opportunity of expressing my thanks for much kind advice, that in the Acropolis at Athens, such examples of coloration are to be seen as a blue bull, a blue horse, a man with blue hair, beard and mustache, these probably dating from 600 B. C., certainly later than the time of Homer.* When such examples of eccentric coloration in blue are found associated with the defect in nomenclature for the same color, it is difficult to believe that the sense for this color can have been as highly developed in those times as it is among civilized races at the present day. The whole subject of the use of color in ancient monuments, in its bearing on color vision, requires a more thorough investigation than it has hitherto received.

Another line of objection to the views of Gladstone and Geiger, which has already been mentioned as having been taken up by Grant Allen, is derived from the high degree of development of the color sense in many of the lower animals, and especially in insects and birds. To many of those who have taken part in the controversy, this objection appears to have been regarded as conclusive. A well-developed color sense in any one branch of the animal kingdom does not, however, necessarily imply the existence of the same in other, even if higher, forms. We have many instances of the independent development of closely similar mechanisms in very widely separated branches of the animal kingdom, and there is nothing improbable in the view that this may have been so in the case of the color sense. If the color sense were found to be highly developed in mammals, the fact would naturally have a closer bearing on the color sense of man than has the presence of a similar development in birds. The evidence, however, of such development in mammals is very defective. Graber,† who has carried out the most comprehensive investigations of the color sense in different branches of the animal kingdom, obtained much less definite evidence from mammals than from other animals, and altogether failed to obtain evidence in the case of some species. Again, if the anthropoid apes were found to have a well-developed color sense, the fact would have

* See also Gardner, 'Handbook of Greek Sculpture,' p. 28.

† 'Grundlinien zur Erforschung des Helligkeits und Farbensinnes der Tiere.' Prag., 1884.

a still closer bearing on the condition of primitive man, but here again the scanty evidence is negative. The only experimental investigation with which I am acquainted is that made by Romanes* on the chimpanzee 'Sally' in the Zoological Gardens. After having successfully taught this animal to recognize numbers, Romanes proceeded to apply a similar method to teach her colors, but wholly without success, and he was obliged to conclude that the animal was probably color-blind. It may be objected that the brilliant coloration of the mandrill and other species points to the existence of a color sense in the primates, but little weight can be attached to such indirect evidence in the absence of experimental investigation.

Another subject which has some bearing on the question is that of the color sense of the human child. It is now a more or less accepted principle in biology that the history of the individual presents the same stages of development as have occurred in the history of the race. Darwin† was the first to point out that the power of distinguishing colors is a very late accomplishment in childhood; he found that his children were unable to name colors correctly at an age in which they knew the names of all familiar objects. This subject has since been the subject of much investigation, the most important work having been done by the late Professor Preyer‡ and by Garbini.§ Preyer made a very large number of investigations on one child, while Garbini has based his results upon the observations of no less than 600 children. Both agree in the conclusion that the child is unable to distinguish colors at all till towards the end of the second year, and they also agree that red is distinguished and named correctly at an earlier age than blue, although there is some difference of opinion as to the exact order of development of other colors. Garbini points out further that the power of distinguishing colors develops earlier than the power of naming colors, language appearing to lag behind sense. If any importance is to be attached to the bearing of the history of the child on the history of the race, the evidence from childhood is in favor of the view that the color sense of man is a comparatively recent acquirement.

Whatever room for difference of opinion there may be on the question of the evolution of the color sense, there can be no doubt that there has been an evolution of color language. The possibility that the course of this evolution has been determined by physiological conditions has been considered, but there can, I think, be little doubt that these have not been the only factors upon which the characteristic defects of language have depended. The deficiency in the sense for blue, which

* 'Proc. Zoolog. Soc.,' 1889; p. 316.

† 'Kosmos,' Bd. I., s. 376; 1877.

‡ 'Die Seele des Kindes.' Leipzig, 1884.

§ 'Arch. per l'Anthropologia e la Ethnologia,' Vol. XXIV., pp. 71 and 193; 1894.

I found in Torres Strait, is only partial and can not wholly account for the absence of a word for that color. Even to those with normal sensitiveness to blue, I think, there is no doubt that there is a closer resemblance between blue and black and between green and black than between red and black, and this difference in the degree of similarity between the different sensations of color and that of blackness may account in some measure for the difference in the definiteness of nomenclature.

It is a characteristic of the language of primitive races to have special names for every natural object, and often for very many individual parts of a natural object. If the savage has one name for one blue flower and another name for another, and so on, he will not require a name for the abstract quality of blueness. It is possible that he only begins to require names for colors when he begins to use pigments. If this be the case, it may help to explain the earlier development of names for red and yellow, for in many parts of the world pigments of these colors are by far the most common. In Torres Strait there were both red and yellow pigments, but no green pigment, and the nearest approach to a blue pigment was a slate-colored shale, and there appear to be many parts of the world where a blue pigment is wholly absent. Probably the most widely distributed blue pigment is indigo, and I have endeavored to ascertain whether those races which are familiar with indigo have a word for blue, but the evidence I have at present is too scanty to allow me to express an opinion on this point. It is probable, however, that the distribution of pigments has helped to determine the characteristic features of primitive color nomenclature, the greater frequency of red and yellow pigments being probably one of the factors which account for the more definite nomenclature for those colors.

Another factor, which may have been of importance, is the absence in the savage of an æsthetic interest in nature. The blue of the sky, the green and blue of the sea and the general green color of vegetation do not appear to interest him. It is, however, possible that the sky and sea do not interest the savage, or interest him less than the civilized man, because their colors are less brilliant than they are to us, and consequently this factor is not one on which much stress can be laid.

The widespread defect in the nomenclature for blue is rendered more striking by the fact that a name for red is universally present in primitive languages, while in many languages, as in that of Murray Island, various shades of red are not only discriminated, but also receive special names. In the experiments made in Torres Strait it seemed to me that this definiteness in the nomenclature for red was associated with a high degree of sensitiveness to this color, apparently

greater than that of the average European. I also found, both by observation of their clothing and by direct questioning, that red was the favorite color of these people. In reading accounts of primitive man, one can not help being struck by the great predominance of red in the decoration of their houses, weapons and implements. This predominance may partly be due to the striking nature of the color and also to the prevalence of red pigments, but it seems possible that it may also be connected with the fact that red is the color of blood. Many savage races appear to be in a state of constant warfare, and in the religious rites and ceremonies of nearly all primitive races blood pays a great part.

The suggestion may even be hazarded that the earliest use to which red pigments were put was to smear the body in the war-dance, to imitate the blood-stained victor,* or to replace blood in the various ceremonies of which it so often forms an essential feature. In his 'Legend of Perseus,'† Mr. Hartland has collected a number of instances in which it is perfectly obvious that vermilion or other red pigment has been used in the place of blood. Both in Murray Island and Mabuiag the chief words for red were derived from the name for blood, and this derivation is found in many languages, including our own. To whatever cause it may be due, there is no doubt that red is the most important color in the life of the savage, and it is natural that the predominant color should also be that which has the most definite name.‡

The main conclusions may be summed up as follows: The language used for color in ancient writings shows a characteristic defect, from which it has been concluded that the color sense of ancient races was also defective.

Existing primitive races agree in showing the same defect of color language as is found in ancient writings, and, in at least one such race, there has been found to be a corresponding defect in color sense, consisting in a certain degree of insensitiveness to those colors for which the nomenclature is defective.

Evidence, derived from ancient monuments and from the color vision of animals, which has been held to disprove the existence of any defect in the color senses of the ancients, appears to be inconclusive, and might, indeed, be held to support the views which have been derived from the study of language.

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* This custom appears to have persisted down to Roman times, in which the conqueror painted his body red when taking part in the triumphal procession.

† Vol. II., pp. 242, 264; notes, 342, 354-5.

‡ Since the above was written, the question of the predominance of red has been fully discussed by Havelock Ellis. (POPULAR SCIENCE MONTHLY, August and September, 1900.)

The observations made on the color vision of childhood may be regarded as indirect evidence that color vision has been a comparatively recent acquirement of the human race.

In addition to possible physiological conditions, there are certain other factors which may have taken part in the production of the characteristic features of primitive color language.

On the more special question of the color sense of Homer, I believe that Gladstone and Geiger went too far. The evidence seems to me to suggest one of two possibilities. It is possible that to the Greeks of the time of Homer green and blue were less definite, possibly duller and darker colors than they are to us. It is, however, possible that the language used by Homer was only a relic of an earlier defect of this kind, the defect of nomenclature persisting after the color sense had become completely developed, language lagging behind sense in the race, as it appears to do in the child. According to the latter view, the defective terminology of Homer would be a phenomenon of the same order as the absence of a word for blue in such languages as Welsh, Chinese and Hebrew at the present day. It would not necessarily show the actual existence of a defective color sense, but would suggest that at some earlier stage of culture there had been defective sensitiveness for certain colors.

The evidence derived from poetry and art must always be in some degree unsatisfactory, owing to the great part which convention plays in these productions of the human mind. Still, every convention must have had a starting point, and though, in some cases, it is possible that considerations of technique* may have originated the conventional use of color, it seems more probable that the predominance of red and deficiency of blue, both in the color language and in the decoration of the ancient Greeks, however conventional they may have become, nevertheless owe their origin to the special nature of the development of the color sense.

The subject of the evolution of the color sense in man is one which can only be settled by the convergence to one point of lines of investigation which are usually widely separated. The sciences of archæology, philology, psychology and physiology must all be called upon to contribute to the elucidation of this problem. I do not wish to do more than reopen the subject, and shall be contented if I have shown that the views of Gladstone and Geiger cannot be contemptuously dismissed as they were twenty years ago.

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* As an instance of the origin of a convention in technique, Mr. Sikes has suggested to me the red figures on Greek vases. Early Greek vases were made of a reddish material, on which the figures were designed in black. At a later time, the vessels were black and the figures red, the conventional persistence of red decoration in this case having had its starting-point in the special nature of the material originally used in the manufacture of the vases.

A STUDY OF BRITISH GENIUS.

BY HAVELOCK ELLIS.

V. CHILDHOOD AND YOUTH.

IF we consider the time of birth of our group of British persons of preeminent ability we find that April shows the largest number of births and January the fewest number. In passing from January to February there is a marked and sudden rise, so that when we consider the total births, according to the quarter of the year, the first, second and fourth quarters are fairly equal, but there is a decided deficiency in the third quarter. This is not quite the result which we find on considering the birth-rate among the ordinary population of England and Wales during the nineteenth century. Here the birth-rate during the first and second quarters agrees in being very high, while the third and fourth quarters invariably show a low rate. The discrepancy is in the fourth quarter, persons of preeminent ability being born during that quarter in unduly high proportion. In order to reach the time of conception, and so consider the possible significance of these facts, we must, of course, push these periods three months forward.*

The first significant fact we encounter in studying the life-histories of these eminent persons is the frequency with which they have shown marked constitutional delicacy in infancy and early life.† A group of at least five—Joanna Baillie, Hobbes, Keats, Newton, Charles Wesley—were seven months children, or, at all events, notably premature in birth; it is a group of very varied and preeminent ability. Not including the above (who were necessarily weakly), at least eight are noted as having been very weak at birth, and not expected to live; in several cases they were, on this account, baptized on the same day. In addition to these, fifty-five are described as being of very delicate health in infancy or childhood. Further, we are told of sixty-nine

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* For a discussion of the normal phenomena, see H. Ellis, 'Studies in the Psychology of Sex,' 'The Phenomena of Sexual Periodicity.'

†Mr. A. H. Yoder ('Pedagogical Seminary,' October, 1894) stumbled across this fact in the course of his interesting study of the early life of a group of men of genius, but failed to realize its significance. He put it aside as due to a desire on the part of biographers to magnify the mental at the expense of the physical qualities of their subjects. There is no evidence whatever for this gratuitous assumption.

others that throughout life they suffered more or less from chronic ill-health, so that we may assume that in most cases their feeble constitutions were congenital and existed at birth. Thus, at the lowest estimate—for we may be certain that the national biographer has very frequently overlooked the point—137 of the 902 British men and women of preeminent intellectual ability (or 15 per cent.) were congenitally of notably feeble physical constitution.

Although it may fairly be assumed that this proportion, at least, of our eminent persons showed signs of physical inferiority at the beginning of life, it must not be assumed that in all cases such inferiority was marked throughout life. The reverse of this is notably the case in many instances. This is not indeed absolutely proved by longevity, frequently noted in such cases, for men of genius have sometimes lived to an advanced age, though all their lives suffering from feeble health. But there is a large group of cases (probably much larger than actually appears), in which the delicate infant develops into a youth or a man of quite exceptional physical health and vigor. Bruce, the traveler, is a typical example. Very delicate in early life, he developed into a man of huge proportions, athletic power and iron constitution. Jeremy Bentham, very weak and delicate in childhood, became healthy and robust and lived to eighty-four; Burke, weak and always ailing in early life, was tall and vigorous at twenty-seven; Constable, not expected to live at birth, became a strong and healthy boy; Dickens, a puny and sickly child, was full of strength and energy by the age of twelve; Galt, a delicate, sensitive child, developed Herculean proportions and energy; Hobbes, very weak in early life, went on gaining strength throughout life and died at eighty-one; Lord Stowell, with a very feeble constitution in early life, became robust and died at ninety-one. It would be easy to multiply examples, though the early feebleness of the future man of robust constitution must often have been forgotten or ignored, but it is probable that this course of development is not without significance. I have noted those cases in which one or both parents have died soon after the birth of their eminent child. One small, but eminent, group—Blackstone, Chatterton, Cowley, Newton, Adam Smith, Swift—had lost their fathers before birth. By the age of five at least fifty-five of these eminent persons had lost their fathers and thirty-one their mothers. By the age of ten at least eighty-eight had lost their fathers and fifty their mothers. In fourteen of these cases both parents were dead. So that over 14 per cent. had lost one or both parents by the age of ten. It is difficult to estimate the real extent of this tendency on account of the imperfect nature of the data, nor have I any data at hand for normal families. In New Zealand a useful enactment requires the ages of living children to be inserted in the parent's death certificate.

Full particulars are not given by the Registrar-General of New Zealand in any report in my possession, but it appears that at least 45 per cent. of New Zealand children whose fathers died under the age of sixty-five were under fifteen at the time. This, of course, does not tell us what proportion these children bear to the children of the same age whose fathers are living. In English reformatories, Douglas Morrison notes, as a very high proportion, that 33 per cent. of the children admitted under the age of sixteen had lost one or both parents.

The chief feature in the childhood of persons of eminent intellectual ability brought out by the present data is their precocity. This has indeed been emphasized by previous inquirers into the psychology of genius, but its prevalence is very clearly shown by the present investigation. It has certainly to be said that the definition of 'precocity' requires a little more careful consideration than it has sometimes received at the hands of those who have inquired into it, and that when we have carefully defined what we mean by 'precocity' it is its absence rather than its presence which ought to astonish us in men of genius.* Judging from the data before us, there are at least three courses open to a child who is destined eventually to display preeminent intellectual ability. He may (1) show extraordinary aptitude for acquiring the ordinary subjects of school study; he may (2), on the other hand, show only average, and even much less than average, aptitude for ordinary school studies, but be at the same time engrossed in following up his own preferred lines of study or thinking; he may, once more (3), be marked in early life solely by physical energy, by his activity in games or mischief, or even by his brutality, the physical energy being sooner or later transformed into intellectual energy. It is those of the first group, those who display an extraordinary aptitude for ordinary school learning, who create most astonishment and are chiefly referred to as proving the 'precocity' of genius. There can be no doubt whatever that even in the very highest genius such extraordinary aptitude at a very early age is not infrequently observed. It must also be said that it occurs in children who, after school or college life is over, or even earlier, display no independent intellectual energy whatever. It is probable that here we really have two classes of cases simulating uniformity. In one class we have an exquisitely organized and sensitive mental mechanism which assimilates whatever is presented to it, and with development ever seeks more complicated problems to grapple with. In the other class we merely have a sponge-like mental receptivity, without any corresponding degree of aptitude for intellectual organization, so that when the period of

* For a summary of investigations into the precocity of genius, see A. F. Chamberlain, 'The Child,' pp. 42-6.

mental receptivity is over no further development takes place. The second group, comprising those children who are mostly indifferent to ordinary school learning but are absorbed in their own lines of thought, certainly contains a very large number of individuals destined to attain intellectual eminence. They by no means impress people by their 'precocity'; Scott, occupied in building up romances, was a 'dunce'; Hume, the youthful thinker, was described by his mother as 'uncommon weak-minded.' Yet the individuals of this group are often in reality far more 'precocious,' further advanced along the line of their future activities, than the children of the first group. It is true that they may be divided into two classes, those who from the first have divined the line of their later advance, and those who, like the youthful Diderot, are only restlessly searching and exploring; but both alike have really entered on the path of their future progress. The third group, including those children who are only noted for their physical energy, is the smallest. In these cases some powerful external impression—a severe illness, an emotional shock, contact with some person of intellectual eminence—serves to divert the physical energy into mental channels. In those fields of eminence in which moral qualities and force of character count for much, such as statesmanship and generalship, this course of development seems to be a favorable one, but in more purely intellectual fields it scarcely seems to lead very often to the finest results. On the whole, it is evident that 'precocity' is not a very valuable or precise conception as applied to persons of intellectual eminence. The conception of physical precocity is fairly exact and definite. It indicates an earlier than average attainment of the ultimate growth and maturity. But we are by no means warranted in asserting that the man of intellectual ability reaches his full growth and maturity earlier than the average man. And even when as a child he is compared with other children, his marked superiority along certain lines may be apparently more than balanced by his apparent inferiority along other lines. It is no doubt true that, in a vague use of the word, genius is very often indeed 'precocious'; but it is evident that this statement is almost meaningless unless we use the word 'precocity' in a carefully defined manner. It would be better if we asserted that genius is in a large number of cases mentally abnormal from the first, and if we were to seek to inquire precisely wherein that mental abnormality consisted. With these preliminary remarks we may proceed to note the prevalence among British persons of genius of the undefined conditions commonly termed 'precocity.'

It is certainly very considerable. Although we have to make allowance for ignorance in a large proportion of cases, and for neglect to mention the fact in many more cases, the national biographers note that

223 of the 902 eminent persons in our list may in one sense or another be termed precocious, and only thirty-seven are mentioned as not precocious. Many of the latter belong to the second group, as defined above—those who are already absorbed in their own lines of mental activity—and are really just as 'precocious' as the others; thus Cardinal Wiseman as a boy was 'dull and stupid, always reading and thinking;' Byron showed no aptitude for school work, but was absorbed in romance, and Landor, though not regarded as precocious, was already preparing for his future literary career. In a small but interesting group of cases, which must be mentioned separately, the mental development is first retarded and then accelerated; thus Chatterton up to the age of 6½ was, said his mother, 'little better than an absolute fool,' then he fell in love with the illuminated capitals of an old folio, at seven was remarkable for brightness and at ten was writing poems; Goldsmith, again, was a stupid child, but before he could write legibly he was fond of poetry and rhyming, and a little later he was regarded as a clever boy, while Fanny Burney did not know her letters at eight, but at ten was writing stories and poems.

Probably the greatest prodigies of infant precocity among these eminent persons were Cowley, Sir W. R. Hamilton, Wren and Thomas Young, three of these, it will seem, being men of the first order of genius. Barry and Thirlwall were also notable prodigies, and it would be easy to name a large number of others whose youthful proficiency in learning was of extremely unusual character. While, however, this is undoubtedly the case, it scarcely appears that any actual achievements of note date from early youth. It is only in mathematics, and to some extent in poetry, that originality may be attained at an early age, but even then it is very rare (Newton and Keats are examples), and is not notable until adolescence is completed.

The very marked prevalence of an early bent towards those lines of achievement in which success is eventually to be won is indicated by the fact that in those fields in which such bent is most easily perceived it is most frequently found. It is marked among the musicians, and would doubtless be still more evident if it were not that our knowledge concerning British composers is very incomplete. It is specially notable in the case of artists. It is reported of not less than thirty-five (or in the proportion of over 50 per cent.) that in art they were 'precocious.'

A certain proportion of the eminent persons on our list have followed the third course of early development as defined above, that is to say, they have been merely noted for physical energy in youth. Sir Joseph Banks was very fond of play till fourteen, when he was suddenly struck by the beauty of a lane; Isaac Barrow was chiefly noted for fighting at school; Chalmers was full of physical activity,

but his intellect awoke late; Thomas Cromwell was a ruffian in youth; Thurlow, even at college, was idle and insubordinate; Murchison was a mischievous boy, full of animal spirits, and was not interested in science till the age of thirty-two; Perkins was reckless and drunken till his conversion. It can scarcely be said that any of these remarkable men, not even Barrow, achieved very great original distinction in purely intellectual fields. In order to go far, it is evidently desirable to start early.

The influence of education on men of genius is an interesting subject for investigation. It is, however, best studied by considering in detail the history of individual cases; generalized statements cannot be expected to throw much light on it. I have made no exact notes concerning the school education of the eminent persons at present under consideration; it is evident that as a rule they received the ordinary school education of children of their class, and very few were, on account of poverty or social class, shut out from school education. A small but notable proportion were educated at home, being debarred from school-life by feeble health; a few, also (like J. S. Mill), were specially educated by an intellectual father or mother.

The fact of university education has been very carefully noted by the national biographers, and it is possible to form a fairly exact notion of the proportion of eminent British men who have enjoyed this advantage. This proportion is decidedly large. The majority (53 per cent.) have, in fact, been at some university. Oxford stands easily at the head; 40 per cent. of those who have had a university education received it at Oxford, and only 33 per cent. at Cambridge. An interesting point is observed here; the respective influences of Oxford and Cambridge are due to geographical considerations; there is a kind of educational watershed between Oxford and Cambridge, running north and south, and so placed that Northamptonshire is on the eastern side. Cambridge drains the east coast, including the highly important East Anglian district and the greater part of Yorkshire, whilst Oxford drains the whole of the rest of England as well as Wales. This at once accounts both for the greater number of eminent men who have been at Oxford and for the special characteristics of the two universities, due to the districts that have fed them, the more literary character of Oxford, the more scientific character of Cambridge. The Scotch universities are responsible for 15 per cent. of our eminent men, Edinburgh being at the head, Glasgow and St. Andrews following at some little distance. Trinity College, Dublin, shows 3 per cent. The remaining 8 per cent. have studied at one or more foreign universities, sometimes in addition to study at a British university. Paris (the Sorbonne) stands at the head of the foreign universities, having attracted as many English students as all the other European univer-

sities put together. This is doubtless mainly due to the fact that Paris was the unquestioned intellectual center of Europe throughout the long period of the Middle Ages, though the intimate relations between England and France may also have had their influence. With the revival of learning Italian universities became attractive, and Padua long retained its preeminence as a center of medical study. During the seventeenth century the Dutch universities, Leyden and Utrecht, began to attract English students, and continued to do so to some extent throughout the greater part of the eighteenth century. It was not until the nineteenth century that English students sought out the German universities. Douai might perhaps have been included in the list as the chief substitute for university education for the eminent English Catholics who have appeared since the Reformation.*

While the fact of university education is easily ascertained, it is less easy to define its precise significance. The majority of our men of preeminent intellectual ability have been at a university; but it would be surprising were it otherwise, considering that the majority of these men belong to the class which in ordinary course receives a university education. It would be more to the point if we knew exactly what influence the universities had exerted, but on this our present investigation throws little light. In a considerable number of cases, at least, the university exerted no favorable influence whatever, the eminent man subsequently declaring that the years he spent there were the most unprofitable of his life; this was so even in the case of Gibbon, whose residence at Oxford might have been supposed to be very beneficial, for at the age of fourteen he had already been drawn toward the subject of his life-task. In a large number of cases, again, the eminent man left the university without a degree, and in not a few cases he was expelled. It is evident, however, on the whole, that university life has not been unfavorable to the development of intellectual ability, and that while our eminent men do not appear to have been usually subjected to any severe educational discipline, they have been in a good position to enjoy the best educational advantages of their land and time.

* It may be interesting to compare these results with those obtained by Mr. Maclean in his study of nineteenth century British men of ability. He found that among some 3,000 eminent men, 1,132, or 37 per cent., are recorded as having had an English, Scotch or Irish university education. Of these 1,132, 37 per cent. were at Oxford, 33 per cent. at Cambridge, 21 per cent. at Scotch universities, 7 per cent. at Dublin, and the small remainder were scattered among various modern institutions. It will be seen that university education plays a comparatively small part in this group. This may be in part due to the lower standard of eminence, but it may also be due to the wide dissemination of the sources of knowledge. In no previous century would so encyclopædic a thinker as Herbert Spencer have been able to ignore absolutely the advantages of university centers.

If this is not a very decisive result to reach, there is another less recognized method of educational development which occurs so frequently that I am disposed to attach very decided significance to it. I refer to residence in a foreign country during early life. The eminent persons under consideration have indeed spent a very large portion of their whole lives abroad, whether from inclination, duty or necessity (persecution or exile), and it might be interesting to note the average period of life spent by a British man of genius in his own country. I have not attempted to do this, but I have invariably noted the cases in which a lengthened stay abroad has occurred during the formative years of childhood or youth. I have seldom knowingly included any period of less than a year; in a few cases I have included lengthened stays abroad which were made about the age of thirty, but in these cases those periods of foreign residence exerted an unquestionable formative influence. I have excluded soldiers and sailors altogether, for in their case absence from England at a very early age has been an almost invariable and inevitable incident of their lives, and has not always been of a kind conducive to intellectual development. Nor have I included the very numerous cases in which transference from one part of the British Islands to another has sufficed to exert a stimulating influence of the greatest importance. With these exceptions, we find that as many as 322 of the eminent persons on our list (or about 40 per cent.) during early life, and in all but a few cases before the age of thirty, have spent abroad periods which range from about a year, and in very many cases have extended over seven years, up to extreme cases, like that of Caxton, who went to Bruges in early life and stayed there for thirty years; or Buchanan, who went to France at the age of fourteen and was abroad for nearly forty years. It is natural that France should be the country most frequently mentioned as the place of residence, but France is closely followed by other countries, and a familiarity with many lands, including even very remote and scarcely accessible countries, is often indicated. It may further be noted that this tendency to an association between high intellectual ability and early familiarity with foreign lands is by no means a comparatively recent tendency. It exists from the first; the earliest personage on our list, St. Patrick, was kidnapped in Scotland at the age of sixteen, and conveyed over to Ireland; it seems, indeed, that in the nineteenth century the tendency became less marked, in face of the average modern Englishman's hasty and unprofitable method of traveling. In any case, however, it is evident that there has been a very marked tendency among these men of preeminent ability to familiarize themselves in the most serious spirit with every aspect of nature and life. It is equally marked among the men of every group, among poets and statesmen, artists and divines. It is not least marked in the case of men of sci-

ence from the days of Ray onwards; if it had not been for the five years on the *Beagle* we should scarcely have had a Darwin, and Lyell's work was avowedly founded on his constant foreign tours. In a notable number of cases this element comes in at the earliest period of life, the eminent person having been born abroad and spent his childhood there. The presence of so large a number of our eminent men at a university may be in considerable measure merely the accident of their social position. The persistence with which men of the first order of intellect have sought out and studied unfamiliar aspects of life and nature, or have profited by such aspects when presented by circumstances, indicates a more active and personal factor in the evolution of genius.

THE FROG AS PARENT.

By Professor E. A. ANDREWS,
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IN the life of a common frog or toad we seem to find none of that altruistic solicitude for the welfare of the helpless younger members of society, that we so fondly attribute as guide in many of our own actions. In clamorous spring reunions these cold-blooded creatures deposit their eggs in the water and go their way in search of food—not knowing whether some or many of the eggs will run a normal course through tadpole or pollywog states to tailless adults, or fall a prey to hungry ducks, or more insatiable naturalists in search of 'material' for study.

The naturalists' belief that these Amphibia are closely akin to fish in many ways is borne out in their breeding habits; for, like fish, they have more or less complex 'instincts' that lead the males and females together at the laying season, and then, like fish, they separate till the next period of egg-laying. The eggs, discharged in the water, are fertilized outside the body, and undergo a process of cleavage or cell-multiplication, thus gradually differentiating into active larvæ or tadpoles without any care from the parents. The tadpole leads its own independent fish-like life for months or years, till—if not destroyed—the critical period of transformation arrives. This passed, the young frog or toad has only its instincts to guide it in learning the new life and nothing to learn from its parents—unless perchance they may be near enough to endeavor to swallow it alive.

Yet even here we might fancy some thought for the morrow of the species—the eggs are generally laid in the right place—according to the kind of frog or toad—to have enough water and not too many enemies for the young, while the protecting jelly mass about the eggs is often rather carefully fastened to plants or sticks, thus keeping them near the surface of the water and in optimum conditions for hatching.

But this is not clear until we see some of the extremes to which such prevision for the next generation is carried in certain members of this group. Just as amongst fish there are a few with most remarkable habits—the male stickleback watching and protecting the eggs in his carefully made nest—so, if we look far enough, we find frogs and toads that show most exemplary solicitude for the young. In Europe, Asia, Africa and in South America such curious life-histories are more or less common.

In most such cases the peculiar habit of the parent seems to be associated with an unusual character and development of the eggs. In the common species that have many small eggs, these are left by the parent to develop slowly in the water, where they gradually assume a frog-like character. Whereas species having few and large eggs protect these in some manner until they rapidly turn into frogs with little or none of the aquatic youth we are so used to regard as a *sine qua non* for a frog.

We need go no further than the island of Jamaica for examples of the protected eggs. For there, where everything has to compete strenuously for light and air, the trees themselves support dense populations of plants and these harbor animals of various sorts—amongst them, frogs. One kind of frog in Jamaica lays its eggs in the water that accumulates at the bases of the leaves of Bromelias growing high up on tree trunks, and here the tadpoles have their brief existence.

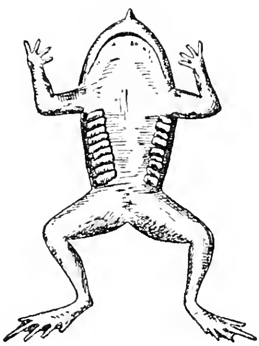

FIG. 1.

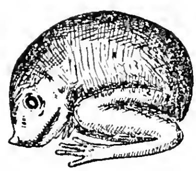

FIG. 2.

Another frog abandons even this semblance of aquatic life, laying a few very large eggs under stones and 'trash' on the ground, where they may even be many miles distant from the water—and the young develop into small frogs that hatch from the egg without having known what it is to be a tadpole. Stevenson's fable admits of literal application here.

'Be ashamed of yourself,' said the frog. 'When I was a tadpole I had no tail.' 'Just what I thought,' said the tadpole; 'you never were a tadpole.'

In this frog, however, there is within the egg a stage when the young is active and has a tadpole form, lacking chiefly the medium in which to express its tadpole possibilities. Not being able to swim with its tail, it yet puts this to good use, for it is richly supplied with blood-vessels and can serve as a breathing organ.

In the Solomon Islands there is another frog (*Rana opisthodon*) which lays large eggs, 6-10 mm. thick, on moist ground and not in

the water; and the young remain within the egg until they are complete frogs in shape. While passing through so much of their life-history within the egg shell, the young manage to breathe, not by their tails, but by special folds of skin that grow out on either side of the belly, in rows, as shown in Fig. 1. The young frog also makes a temporary contrivance for breaking through the egg shell, something like the horn on the beak of a hatching chick or the protuberance used by many reptiles for the same purpose. This peculiar little organ in the frog is shown in Fig. 1 and again in Fig. 2, where it adds a decided retroussé element to a not too intellectual countenance.

We might place frogs in three groups: those that are simply layers, those that are nest-makers, and those that are nurses.*

As a nest builder we may reckon the *Cystignathus mystaceus* of Brazil. This frog makes a hole about as big as a teacup under stones or decayed logs near enough to puddles of water to be covered by water when the pond rises after heavy rains. In this nest the yellow eggs are laid in a mass of thick, white foam, very like beaten white of egg in appearance. The eggs hatch into tailed tadpoles, and, when the water rises over the nest, these young swim off like our common tadpoles. They differ, however, in being able to overtide dry seasons. When the pond dries up and common tadpoles would die, these peculiar creatures gather under boards or logs and there keep moist—apparently by the aid of an unusual amount of material secreted by their skins. Evidently the habits of this frog are nicely adjusted to the climatic conditions under which it lives.

A tree-frog of West Africa *(Chiromantis rufescens)* lays its eggs in leaves of trees in a mass of foamy material that, on drying, hardens on the outside, but becomes liquid within, and so lets the tadpoles swim about for a while till a rain washes them off the tree into the water. While living the short part of its tadpole stage in the nest made by the mother for it, the tadpole has gills such as our common tadpoles breathe with, as well as a tail to swim with. In this aerial existence the young have the protection not only of the surrounding foam, but often of leaves that are sometimes stuck to it. This perfecting of the nest by the use of leaves to envelop the foam mass becomes the rule in two sorts of frog in Brazil *(Phyllomedusa Jheringii* and *Hyla nebulosa)*. The former puts its big white eggs into a mass 40-50 mm. long and 15-20 wide, enveloped by two or three leaves, sometimes willow tree leaves, in such a way that they make a tight case open at one end. The young seem to be so fitted for this peculiar wet-ham-

* The remarkable life histories not in the first group have recently been brought together by R. Wiedersheim, from whose papers in the 'Biologisches Centralblatt' most of the facts and illustrations used in this article are taken.

mock existence that it is doubtful if they need to fall off into the water; at all events, some tadpoles of *Hyla nebulosa* taken out of this nest and put into water, died in a few hours from lack of breath, being unable to live without the peculiar air supply they were used to. However, the larvæ of another frog *(Phyllomedusa hypochondrialis)* are set loose from the nest when the rain softens it and do fall into the water to continue their tadpole life. This species occurs in Paraguay, and makes use of the leaves of trees near the water. The male rides on the back of the female, while both bend in the edges of a single leaf till it makes a funnel. In this the eggs are laid and fertilized. The jelly surrounding them holds the leaf in place till the tadpoles hatch and are ready for the rain to forward them to their new destination.

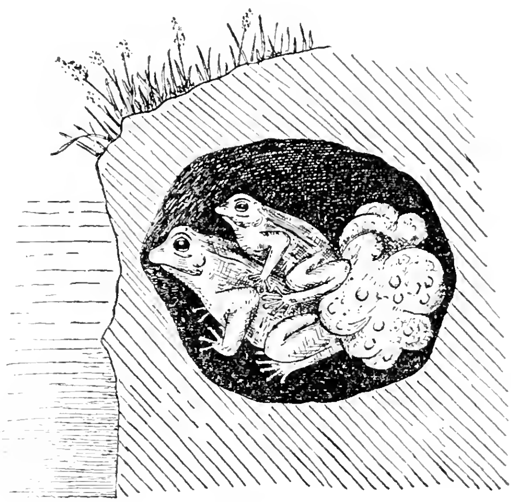

FIG. 3.

In Japan there is also a nest-making frog *(Rhacoporus Schlegeli)* which is said to lay its eggs sometimes amongst leaves on bushes or trees. However, its usual habit is to make a nest in the ground as indicated in Fig. 3. Awakening from their winter sleep, the frogs crawl along the edges of rice fields and swamps and dig out holes above the water level. The female carries the much smaller male, and both become buried in a hole 6-9 cm. wide and 10-15 cm. above the surface of the water. This nest cavity is smoothed inside by the movements of the female and is then, in the night, supplied with a ball of white matter full of air-bubbles. This is tough and elastic and 6-7 cm. thick. This mass is to supply moisture and air for the young. It emerges from the cloaca along with the eggs, and is then kneaded thoroughly by remarkable movements of the feet of the female.

Stretching out and closing the toes, she mixes the sticky mass with air and breaks the big bubbles up into smaller and smaller foam. Similar movements of the feet of the male drive the mass backward and leave the eggs more free for the fertilization that follows. When the eggs have been fertilized and provided with a protecting and aerating mass, the parents break out from the nest and take up their life amongst the trees. The foam mass meanwhile gradually becomes liquid, and flowing out through the hole the parents left on leaving the nest, carries the young into the outside water.

Very different is the nest made by another tree-frog in Brazil. The quaint, beaver-like activities of this creature *(Hyla faber)* are described by one who observed them in his own garden. On a moonlight night one may see a slight movement of the water as if something were moving under it. Then a little mud rises, shoved up by a tree-frog—but only the hands of this are visible. It dives down and again brings up mud;

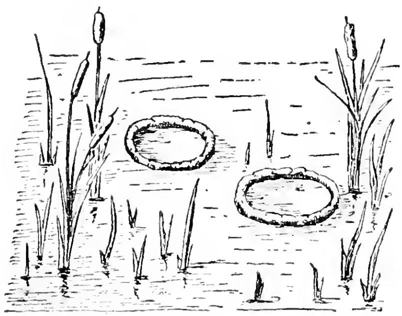

Fig. 4.

gradually the accumulated mud seems a little wall, and eventually it rises 10 cm. above the water and extends as a ring a foot in diameter. Such atolls of mud are shown in Fig. 4. They form little circular dikes of mud elevated above the general expanse of water. The making of them is by no means a matter of chance. The frog—and it is the female that labors, in full enjoyment of her 'rights,' while the male rides passively—uses her hands to compress and to smooth the inside of the wall in a most remarkable way. The top also of the miniature fortress is carefully *manipulated,* the frog crawling out of the water to make all the structure solid and smooth, all but the outer escarpment, which is left rough.

The frog makes the bottom of the crater-like cavity, under water, smooth by gliding along it on its belly, and also by spreading out its hands over it. During all this work the frogs make no sound, though near at hand isolated males may be heard calling their mates.

In this circular nest the spawn is deposited, but not till four or

five days, as a rule, after the nest is completed; there is a period of rest between the nest-making (which in one case required two successive nights) and the egg-laying. The young hatch out as tadpoles and continue to live in the narrow circle of home till the wall be broken down by time and weather to set them free in the world.

It is the habit of all these nest-makers to put their eggs into more or less imperfect nests and then go off and leave them to their fate, the nest, no doubt, increasing the chances of the young passing safely through the earliest stages of infancy—which we all know is a critical period for man or fish. Some other frogs have quite a different habit; carrying their eggs about with them, and so giving them the benefit, if not of actual defense against enemies, at least of passive protection, in that the eggs thus have the same conditions of moisture and concealment which they, the adults, need and obtain for themselves. Such frogs we have called 'nurses.'

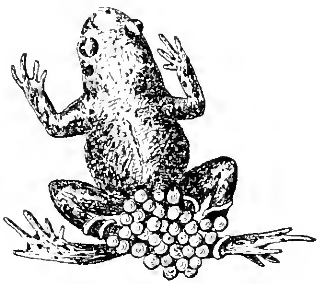

Fig. 5.

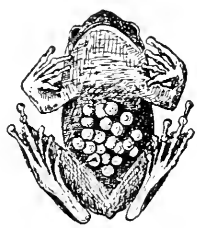

Fig. 6.

Of these nurses the most often mentioned is the so-called 'obstetrical toad' found in Switzerland, France and western parts of Germany. As shown in Fig. 5, the eggs are carried about attached to a band, which is wrapped about the legs of its parent, but the parent who thus carries the offspring about in the moist grass as it seeks food in the evening is not, as one might expect, the mother, but the father toad.

When the eggs are laid and fertilized, the male takes up his burden and carries it till the young are ready to hatch, when he goes to the water and lets the brood escape into the proper element. What leads him there at the right time remains still to be found out. How he comes to assume this care is also not clear; according to some accounts the male, when upon the back of the female, seizes the egg-string first with the right, then with the left foot, and wraps it in figure of eight loops about its own legs. Others say that the male sits behind the female, with its back turned toward her, and as the eggs emerge, the male seizes them between his ankles and, throwing himself now on his back and now on his belly, turns over and over until the egg-string is all drawn out and wrapped about his legs; hence the name *Alytes obstet-*

ricans. Having taken the eggs, however this may be done, the frog emits a clear musical sound and goes hunting its own food, thinking, we may suppose, as little of its offspring as do our own common toads and frogs. The young require a long time to bring them to the hatching stage, and have also an unusually large and peculiar gill on each side.

The tree-frogs of the tropics furnish examples of egg-carrying habit as well as of nest-building. Thus in Ceylon a tree-frog *(Rhacophorus reticulatus)* carries its eggs in a cake-shaped cluster of about twenty firmly fixed to its belly, as indicated in Fig. 6. This time it is not the male, but the female, that aids the coming race by giving it transportation and protection. Probably, however, it is the male and not the female of a frog of the Seychell Islands *(Anthroleptes Seychellensis)* that carries about its young on its back. This is a most complex case, for the eggs are laid upon moist earth or rotten logs and kept moist by the body of the frog until they hatch; they have large tails,

Fig. 7.

Fig. 8.

and even the beginning of hind legs. Then the youngsters get up on the back of their parent and stick there till their development is complete. The peculiar little tadpoles have great power of adhesion, and can stick to the sides of a glass dish as they do to their parent's back (Fig. 7). Another such case occurs in Trinidad and in Venezuela. When the ponds dry up the young tadpoles of the frog *Phyllobates trinitates*, which are as yet without legs, though they have tails, stick themselves firmly to the back of the male frog (whether it is their father or the father of some other tadpoles does not appear), and in this way are carried 'pick-a-back' to some larger pond. Similar habits have been observed in the frogs *Dendrobates trivittatus* and *Dendrobates braccatus*.

Again, a frog *(Hylodes liniatus)* of Dutch Guiana is found with its tadpoles attached to its back, as seen in Fig. 8. The young, from a dozen to twenty, are attached, as shown, with their heads turned towards the middle of the mother's back and do not fall off, even when she leaps rapidly away. Thus the mother, and not the

father, carries the young with her. In the Brazilian tree-frog *(Hyla Goeldii)*, it is again the mother who bears the young with her. The eggs are very large and whitish and crowded together into a rounded mass on the back of the female, as represented in Fig. 9. While all the other frogs as yet mentioned seem to have no special organ or apparatus for holding the eggs or tadpoles as they carry them about with them, this Brazilian frog is provided with a growth of skin on its back to form a wall all around the eggs so that they lie in a spoon-like depression. The knowledge of such a change in the skin of the back for egg-carrying may make us more ready to receive the accounts of the Surrinam toad described by Mlle. Merrain in 1705. When, in 1725, the Dutchman Ruysch described the remarkable pits for carrying young which this creature has upon its back, the account met with natural skepticism, but at the present day reiterated observations place

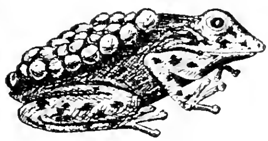

FIG. 9.

FIG. 10.

the main facts beyond doubt. The female of this toad *(Pipa dorsigera)*, as seen in Fig. 10, bears its young upon its back, each in a separate case, like so many papooses.

The cases are made at the breeding season, and before that the two sexes look alike. How the eggs get into these special receptacles is still an unanswered question, though the male may be a factor in the case; at all events, when the male goes away after being some twenty-four hours on the back of the female, the spawn is found on the back of the female, where the eggs gradually sink down into circular pits, that are hollowed out in the skin, and are 10-15 mm. deep. When an egg has sunk down into one of these pits, a thick, leathery or horny roof forms over it, and thus shuts it out from the external world.

These roofs are 5-6 mm. thick, and have a dark color unlike the rest of the back. Whether they, like the rest of the chamber about the egg, are formed from the skin, or whether they may be modified rem-

nants of the mass laid with the egg, is as yet undecided. The part of the skin of the back not taken up by these pits rises up in small papillæ, and thus each pit is closely surrounded by a ring of papillæ. From 40-114 pits are formed, and 60-70 young are developed.

Each egg thus develops inside a diminutive womb-like chamber, on the back of the mother, but as yet we do not know whether the mother supplies any nutriment to the young while it is in this protecting chamber; the arrangement of lymph and blood vessels in the skin of the mother and the organization of the young, however, raise the question whether this may not be the case. In each pit the young tadpole lies with its back toward the roof and its belly downwards. For awhile it has a large yolk-bag, or enlargement of its belly, full of nutriment and richly supplied with blood vessels; also a tail. This tail is very large, and seems to be of no use, unless it may function as a breathing organ, as in the frog of Guadaloupe, mentioned above. Another peculiarity of these favored tadpoles is that they obtain their

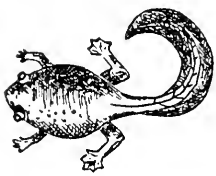

Fig. 11.

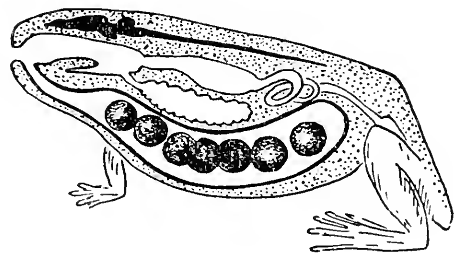

Fig. 12.

front legs at a very early period, before their gills grow out; and thus, for a long time, appear as four-legged creatures, with most imposing tails, as represented in Fig. 11.

In this condition they break off the roofs of their little houses and each, like a baby kangaroo from its mother's pouch, peers out into the world with goggle eyes and ready hands. For nearly three months (eighty-two days) the mother is burdened like Sindbad, till the young toads jump out of their cradles and go free to shift for themselves.

Every Surrinam toad is thus launched upon its individual voyage of contest for food as a complete but small toad, and, though it had the outward symbols of tadpole life—gills and a tail—it never lived a tadpole life, with its dangers from the drying of ponds, and the chances of being swallowed alive as so many tadpoles are. Our common tadpoles may, from one point of view, be regarded as chiefly feeding phases, like the caterpillar that eats and eats to pass through the inactive chrysalis stage to the complete butterfly life. Tadpoles are phases of the frogs' lives, when almost anything can be eaten, and when there

is but little perfection of the senses as compared with the future adult stage. The experience gained as tadpole would seem to be of very little direct use to the frog when it begins its new life; it is chiefly the gain in material, the results of feeding, that makes the frog the better for its tadpole life. In the Surrinam toad this gain in size is provided for by the mother, who takes all labor off the shoulders of her offspring till they are toads like herself. Is there not an analogy between such habits and our own attitude towards the next generation? The child of the savage, or of the more unfortunate classes in 'civilized' communities, is put to getting its own livelihood at the earliest possible age; the favored child of the few is protected by the parental roof and fed with even university pabulum till nearly arrived at adult structure.

But the Surrinam toad is not the only one that is built mammal-like, to carry its young within its own body; the group of pouched or marsupial frogs of Venezuela have an even stranger contrivance for this purpose. Those who are fortunate enough to have access to a copy of Professor Davenport's profusely illustrated little volume, 'Introduction to Zoology,' will find a very attractive figure of one of these 'brooding tree-frogs,' taken from a water-color painting at Harvard College. On the middle of its back, above the loins, is a very large opening leading into the interior of the animal. This is the opening of a large brood-chamber. In one frog *(Nototrema oviferum)* this chamber continues forward on each side as two, even larger, chambers that reach almost to the head. The middle chamber is on the back, while the side chambers extend not only over the back, but down on the side, so as almost to meet one another across the belly. All these chambers lie just beneath the skin and are not deep, but flat, though they displace the viscera.

The walls of the chambers are very thick and vascular, except near the external opening, where the wall is of the same nature as the skin. The skin, in fact, seems to have grown in to line these chambers, but has been much changed in its character in those parts of the chambers that are remote from the openings. It is not known how this big bag grows over the body, nor whether it is always there, or only developed at the breeding season. These chambers are found in the female, and in some unknown way the eggs are transferred from the ovary into the pouches. As there is no internal opening to the pouch, the eggs must be laid as usual and then put upon the back, and so into the external opening of the brood pouch. Possibly the male aids in this function.

The eggs of this species are exceedingly large, being 1 cm. in diameter, more than eight times the bulk of a common frog's egg, and are also but few in number. In one case there were only four eggs in the outer, middle, chamber and eleven others in the two side chambers—fifteen eggs in all.

The young that develop from these comparatively large eggs inside this peculiar skin-bag are remarkable enough to satisfy the ideals of so bizarre a parent. Like the young Surrinam toad, these get their front legs at a very early period, and at an early period are also found without the adhesive organs and horny jaws that seem so essential to all common tadpoles. But their chief departure from received tadpole style is the phenomenal character of the gills. These are large, bell-shaped, flower-like membranes that envelop the tadpole like a mantle and, coming between it and the walls of the mother's pouch, may serve as a means of getting oxygen and possibly food from the mother, for the gills are richly supplied with blood-vessels, and the walls of the mother's pouch are also vascular, giving the anatomical conditions for interchange such as takes place in a mammal's placenta. Each of these two gills seems to have been made by the fusing of two specialized gills, and each retains two stalks. Eventually these big gills are probably lost and replaced by inside gills, just as in common tadpoles the outside gills are always followed by inside gills. Whether the mother goes into the water and lets the young escape where they can use their gills, or whether she keeps them at home till they have lost these youthful structures and can 'come out' in budding maturity, is not known, in this case. But in *Nonotrema marsupiatum* and *Nototrema plumbeum* the young are set free into the water when they are still tadpoles.

The way in which such a capacious pocket on the back can have come about is perhaps indicated by the state of things in *Nototrema pygmæum*. The brood pouch is here small and slit-like, and when the young are ready to leave it they press and wriggle till the pouch is torn open, from the external opening forwards. There are only four to seven young that can come forth in this partially Cæsarian way, and their appearance seems to have been prearranged by the way in which the pouch is made. Two folds of the skin grow up to meet one another along the back, and when they fuse they leave a sort of seam, which is also the line along which the pouch ruptures to let the young out. The pouch of this tree-frog is thus to some degree intermediate between the simple cup on the back of *Hyla Goeldii*, not well shown in Fig. 9, and the more perfect sac of the other pouched frogs described above, and may indicate the lines along which structures and habits like those of *Hyla Goeldii* could have been evolved in those of *Nototrema oviferum*. On the other hand, the small size of this frog and the large size of its eggs make it both impossible to get the eggs into the usual external opening of the pouch and impossible for the big larvæ to escape through it; hence it may be that the pouch grows up as folds after the eggs are laid on the mother's back, and that these folds remain easy of separation to allow the young to escape, all independent

of any mode of development of the pouches of other marsupial frogs. These strange skin pits and bags of the Surrinam toads and the Venezuelan frogs are both an aid to the young and an inconvenience to the parent, and it seems in keeping with our general experience that it is the female that has these special organs and the female that suffers the dangers that go with the prolonged care of the offspring. In both respects, however, with regard both to the phenomenal nature of the breeding organs and in the amount of personal sacrifice, this female is outdone by the male of a little frog of Chili.

This frog is not much more than an inch long, and was first found by Darwin on the voyage of the 'Beagle.' In some unknown way the large eggs get into the mouth of the male and are carried a long time inside a huge sac that opens only into the front part of the mouth. In this pouch the young develop their legs and small tails. It is probable that they remain protected within the male till they are complete lung-breathing frogs and then get out of his mouth and escape. Why he does not eat them is a question that might naturally occur to one knowing only our common frogs.

The brood-sac of this male extends over the throat and belly, back to the loins and up on each side nearly to the backbone. The eggs and young that are found in it are from five to fifteen in number, and lie scattered about in the capacious chamber. The general anatomical relations of this sac are shown in the rude diagram, Fig. 12. This represents some of the organs that would be seen on cutting the frog into halves, lengthwise. The brain and spinal cord along the back are shown in black. Below this are the mouth, stomach and intestines. On the floor of the mouth is an elevated region, the tongue; behind this is the opening to an irregular cavity, one of the lungs. In front of the tongue is the opening to a very large sac, the brood-sac, in which the eggs are represented as large balls.

A bag of this size necessarily causes the skin of the throat to bulge out and also presses upon the internal organs. It is found that even the bones of the shoulder girdle and chest are modified in connection with this remarkable organ, and that the stomach, liver and intestines may be pressed out of place, so that feeding must be difficult. In some cases the digestive organs are said to be so impaired as to be of no use, while in other cases the brood-sac is of much less extent and would seem not to interfere seriously with feeding and digestion.

The brood-sac lies free under the skin, except in certain regions of the throat, where it is fastened to the skin. The lining of this sac is a continuation of the lining of the mouth, in fact, the sac is but an enormous side pouch from the mouth. In looking for any similar organ in common frogs, we find the single or paired resonance chambers that open into the mouth and serve to give volume to the voice. These

chambers are specially large in the male and most used in the breeding season. This Venezuelan frog, *Rhinoderma Darwinii,* seems to have only an enormous development of this resonance chamber converted to the singular purpose of harboring the young. As far as yet known, this frog leads the van of progress from the selfishness of the common frog to the altruism of the few; but there may remain to be discovered in tropical regions other frogs with structures and habits even more advantageous to the race and inconvenient to the individual.

Granting that all the above accounts of the breeding habits of frogs are reliable, they yet leave many details unknown. Very much remains to be found out before we can know the complete life histories of these remarkable creatures. Till we have a fuller knowledge, any attempt at explanation of the breeding habits and structures of these frogs would seem to be necessarily of a provisional nature.

Though it is difficult to describe these frogs without ascribing to them a far-seeing intelligence, the zoologist of to-day knows little ground for such assumption. Still less does he see in such examples of protection for the good of the race any direct acts of the Deity. He commonly interprets them as the results of the working of natural selection; and granting the potency of this means of evolution, the application to the above life-histories seems not difficult.

But it has been said that science is not concerned with the why, but only with the question, 'What is it precisely that does happen?' Shall we not work for more complete knowledge of the facts in the hope of a clearer view of the interconnection of organisms and environment?

May not our present attempts to understand such problems seem, in the future, as unscientific as does at present the fancy of the Japanese poet, who, centuries since, wrote:

"With hands resting on the ground, reverentially you repeat your poem, O Frog."

RECENT PHYSIOLOGY.

By Professor G. N. STEWART.

EVERY year a mass of original work in physiology, covering from ten to fifteen thousand pages, for the most part of formidable size and closeness of print, is collected in the various special journals of the science, or mingled with kindred, though miscellaneous, dust in the transactions of learned societies, or decently buried at the public expense in government bulletins and official reports. Those four hundred square yards of printed matter embrace, on the average, more than five hundred papers in German, English, French and Italian, without reckoning stray messages in less familiar tongues, such as Russian, Polish, Dutch, Spanish, the Scandinavian languages, the dog Latin of graduation theses, and even, it may be Japanese, Arabic and modern Greek. The great majority of these communications either contain new facts or are directed, often with notable acuteness, to the unfolding of new relations between facts previously established. It is obvious that no survey of recent physiology which is possible within the space at our disposal could pretend to exhaust the contents of its crowded archives even for a single year. I shall try rather to trace the main tendencies, while incidentally mentioning some of the outstanding achievements of recent physiological discussion and research, than to enter in any detail into the results of particular investigations.

Foremost among these tendencies is the study of the structure and functions, and especially the chemical and physico-chemical relations of the individual cell, in which, as has been well said by Bunge, in his brilliant Lectures on Physiological Chemistry, lies ever the riddle of life. While the mode of action of the complex physiological mechanisms, built up by the grouping and chaining together of cells of the same or of different kinds, deserves and has attracted the most assiduous attention, it has become more and more apparent that, as we push our enquiries back, we are always, sooner or later, arrested at the boundary of the cell. We attempt, for example, to explain the mechanism by which the circulation of the blood is maintained and regulated, and up to a certain point we succeed tolerably well. We recognize as the central factor the rhythmically contracting heart which forces the blood through the branching arteries into the netted labyrinth of the capillaries, whence it is again conveyed to the heart by the veins, and thus completes its destined round. We know that the rate and force of the heart-beat and the caliber of the blood vessels are controlled by efferent nerves carrying impulses down to them from centers situated in the medulla oblongata, the portion of the central nervous

system that serves to join the spinal cord to the brain. We are further aware that those centers are in touch with all parts of the body by afferent nerves, along which impulses are continually streaming to the centers. It is thoroughly established that the activity with which the centers discharge impulses along the efferent nerves to the heart and the vessels is modified by the arrival of afferent impulses. And it is fairly well understood how, by the action of this craftily balanced apparatus of nerve-fibers and nerve-centers, the supply of blood to the various tissues is adjusted to their ever-changing needs. But when we ask ourselves what happens in one of the nerve-cells which compose the nervous centers when it discharges an impulse? what that impulse which flies at the rate of a hundred miles an hour along the nerve-fiber really is? what is the precise nature of the actions which it arouses or represses in the muscular fibers of the heart or of the arteries when, in the twinkling of an eye, it impinges upon them? we have to answer that we do not know. We are in exactly the same position with regard to the voluntary contraction of the striped or skeletal muscles by means of which the ordinary movements of the body are executed. The nerve cells in which the impulses originate have been located with considerable precision in the so-called motor region of the brain, which comprises the middle portion of the superficial gray matter of each hemisphere. The tracts of nerve-fibers along which those impulses pass to the muscles have been mapped out. The influence of temperature, tension and other conditions on the muscular contraction has been investigated in great detail. But we are again almost completely in the dark as to the actual nature and course of the events that take place within the envelopes of the nerve-cell, the nerve-fiber and the muscular fiber when a muscle contracts in obedience to the will.

One or two promising clues there are, and these are being vigorously followed. Whenever a nerve or a muscle (or, indeed, for that part, a gland, although the phenomena are best seen in muscle and nerve) enters into a condition of physiological activity, an electrical change is set up in the excited part. In muscle, although not as yet in nerve, certain chemical, thermal and optical changes can also be demonstrated. It is obvious that the study of such phenomena, and especially their quantitative study, under as many different conditions as possible, is essential to the solution of our problem. Accordingly, data of this kind, which, it may be hoped, will some day become the basis of a great generalization, are being diligently gathered. Among the most important of recent contributions to the subject is an elaborate investigation of the electrical changes which accompany muscular contraction by Sir John Burdon Sanderson. By photographing the movements of the mercury in a capillary electrometer connected with the

muscle he has obtained a great series of marvelously beautiful records. Gotch and his pupils, using a similar arrangement, have been able to record the electrical changes in active nerves, even when stimulated by rapidly recurring shocks from an induction coil. It may surprise those who have not followed the progress of technique in the biological sciences to learn the extent to which photography is now applied in physiological research. Pictures of even such feeble vibrations as those which give rise to the sounds of the heart may be obtained by connecting a microphone placed near the chest and the primary coil of an induction machine in the same circuit, and photographing the movements of a capillary electrometer connected with the secondary. Exquisite photographs of the electrical variations occurring in the human heart at each beat, first demonstrated by Waller, have been recently published by Einthoven and Lint.

Loeb, working from another direction, has studied the effect of the ions contained in solutions of certain simple salts on rhythmical contraction in general, and particularly on the rhythmical contraction of the heart. He starts with the observation that a striped muscle in a solution of sodium chloride of a certain strength carries out rhythmical contractions which may last 24 to 48 hours. Salts of lime and of potassium hinder the contractions. Nevertheless the muscle remains longer alive when a small amount of calcium or potassium chloride is added to the sodium chloride solution. He explains the seeming paradox by the hypothesis that the sodium ions are the real stimulus for the rhythmical contractions, but yet exert on the muscle a poisonous influence, which is counteracted by the calcium and potassium ions. He finds support for the idea that the sodium ions are actually poisonous to the living substance in the fact that *Fundulus heteroclitus*—a small marine fish with so marvelous a range of adaptation to its environment that it will live, on the one hand, in sea-water to which sodium chloride has been added to the amount of five per cent., and, on the other hand, in fresh and even in distilled water—will not live in pure sodium chloride solutions of about the same strength as sea-water, but will survive in sodium chloride solutions even twice as strong if a little chloride of calcium or of potassium be added. According to Lingle, one of the pupils of Loeb, sodium ions, while acting as the normal stimulus to the discharge of rhythmical contractions by the heart muscle of the turtle, exert upon it, in the absence of calcium and potassium ions, the same deleterious influence as upon striped muscle, a fact also demonstrated by Ringer and others for the heart of the frog. These are the experiments which that eminent contributor to the gayety of nations, the scientific newspaper reporter, has recently travestied under the caption, 'Discovery of the Elixir of Life in Chicago.' They have yielded fresh

evidence that the differences between muscular fibers, such as those of the heart, which contract normally with what we call a spontaneous beat, and the fibers of the skeletal muscles, which only, under ordinary circumstances, contract when excited through their motor nerves, are not so deep-seated as was at one time supposed, since the addition of simple inorganic bodies to the living muscular substance, or their subtraction from it, can alter its behavior in this regard. The experiments of Langendorff, Porter and others on the action of the isolated hearts of warm-blooded animals, which, after being cut out of the body, can be kept alive for several hours by feeding them through their arteries with warm blood from a reservoir, have strengthened the belief that the essential cause of the heart-beat is to be sought in the muscular fibers and not in the nerve-cells present in certain portions of the organ.

With respect to the nerve-cell, research is at present largely concentrated upon the study of its minute structure. Among the numerous methods of staining employed for this purpose two deserve especial mention: the method of Golgi, which is peculiarly useful for bringing out the processes or branches of nerve-cells, and the method of Nissl, which is of great service in the investigation of the body of the cell. A typical nerve-cell when impregnated with a salt of silver, according to Golgi's method, exhibits a wonderful profusion of bifurcating processes, picked out in black like the sharply shadowed branches of a leafless tree under an electric light. But, however intricately the branches of neighboring cells may mingle and intertwine, they do not in general run into, or fuse with, each other, any more than the interlocking boughs of neighboring trees in a forest. By demonstrating this important fact the method of Golgi has revolutionized our ideas of the architecture of the nervous system.

The significance of the peculiar angular or spindle-shaped bodies in the protoplasm of the nerve-cell, which have been revealed by Nissl's method of staining with methylene-blue, is at present arousing the greatest interest. That they have some important relation to the nutrition of the cell seems evident. For when the latter is severed from that one of its processes (the axone) which constitutes the essential part of the nerve-fiber that springs from the cell, the Nissl bodies break up, and either disappear or are dispersed in the form of very minute granules of stainable material in the protoplasm. At the same time the cell becomes swollen, its nucleus is displaced to one side, and it may even atrophy entirely and disappear. As a rule, however, after several months it recovers its normal structure. The administration of considerable quantities of alcohol and other drugs causes a similar effect on the Nissl bodies. It has been known for half a century that the axone degenerates when cut off from the cell of which it is a process.

The fact that the cell suffers also is a striking illustration of the essential unity of the nerve-cell and all its branches, whether they are long or short—three feet in length, as the axones that run from the lower part of the spinal cord to the foot may be, or a thousandth of an inch, like some which arise and terminate within the gray matter of the cord and brain.

Simpler, at first sight, in their action and organization than nerve-cells or muscular fibers are the gland-cells which secrete the digestive juices. The cells of the kidney which separate from the blood the constituents of the urine, and the cells which line the intestine and are engaged in the absorption of the food appear to be simpler still. And simplest of all are the flat, scale-like cells that line the lungs and have to do with the taking in of oxygen and the elimination of carbonic acid and the similar cells which form the walls of the capillaries and are concerned in the production of lymph. Accordingly we have seen of late years, in connection with researches on the functions of such cells, a revival of formal discussion of the general problem of physiology: whether the vital processes can be completely explained in terms of the laws of unorganized matter. This is a question which has had a singular fate. Answered at certain epochs by an almost unanimous negative, it has emerged again with exery fresh advance in mechanical, physical or chemical knowledge, and for a time has seemed about to be settled in the affirmative. It was so in the seventeenth century when the discoveries of the new geometry and the new mechanics were hailed by Descartes and the iatro-mathematical school who were his lineal descendants, although they denied their parentage, as the key which was to unlock all the secrets of that cunningly devised automaton, the animal body, and particularly to explain its movements. At a later date, the determination of the laws of the diffusion of gases appeared to solve the problem of the passage of gases through the lungs, and the determination of the laws of diffusion of dissolved substances and of endosmosis, the problem of absorption from the intestines. With Ludwig's researches on the formation of urine, secretion seemed about to pass out of the group of mysterious 'vital' phenomena, and to become a mere process of filtration. But always as renewed investigation has brought into clearer light the peculiarities, the wizard tricks, one might almost say, of those rare mechanisms that ply so deftly even in the common business of the bodily machine, the gulf that separates the inorganic from the organized world has opened wide as ever, and physiology has still had to wait for a new Curtius to close it.

Quite recently the experiments of de Vries, Van't Hoff and others on osmosis have supplied further physical data for the solution of this perennial problem, and have, therefore, become the starting point of numerous physiological researches. Among these may be mentioned

a series of studies on absorption from the intestine by Waymouth Reid, which have just been published, in collected form, in the 'Philosophical Transactions of the Royal Society.' Starting with the idea of studying the behavior of the intestinal wall when as many as possible of the physical factors which may be supposed to be concerned in absorption have been eliminated, he endeavored to realize this condition by introducing into the intestine of an animal some of its own blood-serum. When this is done the cells that line the alimentary tube are in contact on one side with blood-serum and on the other with capillary vessels containing blood, the liquid portion of which has the same composition as the serum in the intestine. Under these circumstances there could be no passage of material from the intestines to the blood by diffusion or osmosis, if the intestinal wall acted like an ordinary dead membrane. Reid found, as a matter of fact, that the serum was rapidly absorbed. That this was not due to ordinary filtration, that is, to the squeezing of the liquid through the walls of the tube, follows from the fact that in these observations the pressure in the intestines was less than in the capillaries. He comes to the conclusion that while known physical forces play a certain part in absorption, there remains an unexplained residuum. But he refuses to speculate as to the cause of the peculiar endowments of the intestinal epithelium, and is very careful to point out that what seems so inexplicable now may later on become susceptible of explanation.

Friedenthal, in a suggestive paper occupied mainly by a critique of previous work and contemporary speculation, has lately taken up his parable in favor of a complete physico-chemical explanation of absorption. According to him, what we call the selective power of the intestinal epithelium is simply the expression of the fact that there exist in those cells substances which have a greater 'affinity' for certain constituents of the intestinal contents than for others, just as plates of gelatine do not take up the same quantities of different salts and other compounds from solutions containing them. Such hypotheses, of course, while they have the merit of directing attention to the possibility of a complete chemical or physical solution of the problem being some day found, do not give us any information as to the peculiarities of physical structure or chemical composition which confer on the lining of the intestine, as on all living cells, powers so remarkable that when we endeavor to describe them the terms which spring spontaneously to our lips are such as we should apply to the behavior of an entire organism in relation to its environment: 'selection,' 'discrimination,' 'affinity' for substances that are useful, 'antagonism' to those which are injurious.

The study of the permeability to various substances of what we may perhaps consider as the most simply organized cells in the whole body, the colored corpuscles of the blood, promises to throw a flood of light

on absorption in general. It has been lately shown that they are practically non-conductors of electricity in comparison with the liquid portion of the blood, or plasma, in which they float. This is due to the fact that the salts of the plasma, whose ions carry the electricity, penetrate the corpuscles with difficulty, sodium chloride, for example, scarcely passing into them at all. On the other hand, they are freely permeable to ammonium chloride, urea and other bodies. The conditions governing the passage of substances into the corpuscles are evidently very different from those which determine the permeability of an ordinary membrane. This is further shown by the fact that by certain methods of treatment the colossal molecules of the red coloring matter of the blood may be caused to escape from the corpuscles, while the much smaller molecules of the inorganic salts remain still pent within them. Such results are of great interest, for they show that cells which, as regards their main physiological office, the conveyance of oxygen to the tissues, seem to be governed strictly by the physical laws of diffusion of gases, appear to exercise a kind of 'selection' in the taking up of many substances which have nothing to do with their particular function. The suggestion is scarcely to be avoided that in this case a purely chemical or physical 'attraction' underlies the apparently selective power. And this idea is strengthened by the fact that all those characteristic reactions of the colored corpuscles can be obtained many hours after the blood has been removed from the body, and, therefore, at a time when their 'vital' activity may be supposed either to have been extinguished or to have undergone a serious diminution.

The absorption of oxygen and excretion of carbonic acid by the lungs have long been considered conspicuous examples of the passage of substances through a living animal membrane by ordinary physical diffusion. But, according to the recent observations of Bohr, oxygen may, within certain limits, be absorbed, when its partial pressure or tension in the blood is greater than that in the air contained in the lungs, and carbonic acid may be excreted when its pressure in the blood is less than that in the air of the lungs. Haldane and Smith have indeed shown that in man the pressure of the oxygen in the arterial blood is actually higher than in the outside air. These results are, of course, incompatible with a simple theory of diffusion, and show that the cells of the pulmonary membrane have the power of forcing oxygen to move in one direction and carbonic acid in the other even against the slope of pressure.

As regards the physiology of particular organs, attention has been, in recent years, attracted in a marked degree to two subjects: the so-called internal secretions of certain glands and the arrangement and actions of the nerve-cells and fibers which make up the central nervous system.

By an internal secretion we mean a substance or substances

formed by a gland and taken up from it by the blood or lymph. An ordinary external secretion is discharged by a special duct into the proper receptacle, bile, for example, into the gall-bladder, and ultimately into the intestine; urine into the urinary bladder, and so on. Some of the glands which produce important internal secretions have no ducts. Such are the thyroid glands, two insignificant looking reddish bodies situated in the neck, one at each side of the windpipe, a little below the larynx. It had been long known that disease of these glands, commencing in childhood and leading to the enlargement which we call goitre, was often associated with a condition of idiocy (cretinism). Interest in their functions was greatly stimulated by the discovery that excision of the thyroids was followed by grave changes resembling those found in a disease called myxœdema, and that the symptoms produced by excision, as well as those present in the natural disease, could be removed, and health restored, by feeding the patient with the raw or slightly cooked thyroids of animals or with certain extracts prepared from them. Much work has been devoted to the isolation in a pure form of the active substances, one of which contains iodine as an important constituent. It appears to be the office of the thyroid to manufacture for the use of the body a constant supply of these substances, which are necessary for the due maintenance of certain of its functions. In the absence of the natural supply, similar materials produced by the corresponding glands in animals can be utilized.

The suprarenal or adrenal bodies, situated just above the kidneys, are another pair of ductless glands whose function is of extraordinary importance in proportion to their size. It has been shown that they contain a substance which when injected into the blood in animals, or painted, say, on an inflamed eye in man, causes a marked narrowing of the small arteries; and it has been surmised that this substance, oozing slowly from the glands into the blood, exerts a bracing or 'tonic' influence on the muscular fibers of the heart and blood vessels, and helps to keep them in proper condition for their work. Certain it is that death follows their removal in animals, while their disorganization in man is associated with the peculiar and fatal condition termed Addison's disease.

The pituitary gland, a small body attached to the base of the brain, is in the same category. It seems to be of great importance, if not absolutely indispensable to life. Extracts of the gland, as Howell and Schäfer have shown, produce decided effects upon the pressure of the blood when injected into the vessels.

One of the most interesting examples of an internal secretion which is not necessary to life but which yet profoundly affects the chemical changes occurring in the body, is that of the ovaries. It has long been familiar to stock farmers that the removal of these organs greatly

increases the rapidity with which fat is laid on. According to the recent researches of Loewy and Richter at the Agricultural College in Berlin, the explanation is that the ovaries produce a substance which hastens the oxidation of the tissues and the food. When this substance is injected below the skin of animals whose ovaries have been removed, the tissue waste is markedly increased.

In the domain of nervous physiology our knowledge is growing apace. The doctrine of the localization of function on the surface of the brain may now be considered as well established. The motor region has been subdivided into areas, each of which is related to a particular movement, because the nerve-fibers springing from the large pyramidal cells contained in it, are connected with nerve-cells in the gray matter of the spinal cord which send nerve-fibers only to the muscles concerned in that movement. But while each motor center is thus connected by motor or efferent fibers with the muscles, recent work by Sherrington and Mott and by other observers has shown that it is also connected by sensory or afferent fibers with the muscles, the skin overlying them, the joints in their neighborhood, and the bones which they move. The 'motor area,' in fact, is not purely motor, but has sensory functions as well.

No convincing proof has yet been given that any particular portion of the brain is exclusively concerned in intellectual operations. Goltz, the most prominent representative of the dwindling band who still refuse to believe in the localization even of the motor functions, has lately published an interesting paper containing the results of observations on a monkey which was carefully watched for eleven years after the removal of the greater part of the gray matter of the middle and anterior portions of the left hemisphere of the brain. The character of the animal, whose little tricks and peculiarities had been studied for months before the operation, was entirely unaffected. All its traits remained unaltered. On the other hand, disturbances of movement on the right side were very noticeable up to the time of its death. It learned again to use the right limbs, but there was always a certain clumsiness in their movements. In actions requiring only one hand, the right was never willingly employed, and it evidently cost the animal a great effort to use it. Before the operation it would give either the right or the left hand when asked for it. After the operation it always gave the left, till by a long course of training, in which fruit or lumps of sugar served as the rewards of virtue, it learned again to give the right. Evidently, although this is not the interpretation placed by Goltz upon his observations, the motor centers of the right side of the brain, which normally preside over the movements of the left side of the body, had to be laboriously educated before they became able to carry out such movements of the right hand.

THE BLOOD OF THE NATION.

A STUDY OF THE DECAY OF RACES THROUGH THE SURVIVAL OF THE UNFIT.

Part I—In Peace.

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"Over trench and clod
Where we left the bravest of us,
There's a deeper green of the sod."
—Brownell.

I. In this paper I shall set forth two propositions, the one self-evident, the other not apparent at first sight, but equally demonstrable. *The blood of a nation determines its history.* This is the first proposition. The second is: *The history of a nation determines its blood.* As for the first, no one doubts that the character of men controls their deeds. In the long run and with masses of mankind this must be true, however great the emphasis we may lay on individual initiative or on individual variation.

Equally true is it that the present character of a nation is made by its past history. Those who are alive to-day are the resultants of the stream of heredity as modified by the vicissitudes through which the nation has passed. The blood of the nation flows in the veins of those who survive. Those who die without descendants can not color the stream of heredity. It must take its traits from the actual parentage.

II. The word 'blood' in this sense is figurative only, an expression formed to cover the qualities of heredity. Such traits, as the phrase goes, 'run in the blood.' In the earlier philosophy, it was held that blood was the actual physical vehicle of heredity, that the traits bequeathed from sire to son as the characteristics of families or races ran literally in the literal blood. We know now that this is not the case. We know that the actual 'blood' in the actual veins plays no part in heredity, that the transfusion of blood means no more than the transposition of food, and that the physical basis of the phenomena of inheritance is found in the structure of the germ cell and its contained germ-plasm.

III. But the old word well serves our purposes. The blood which is 'thicker than water' is the symbol of race unity. In this sense the

blood of the people concerned is, at once, the cause and the result of the deeds recorded in their history. For example, wherever an Englishman goes, he carries with him the elements of English history. It is a British deed which he does, British history that he makes. Thus, too, a Jew is a Jew in all ages and climes, and his deeds everywhere bear the stamp of Jewish individuality. A Greek is a Greek; a Chinaman remains a Chinaman. In like fashion, the race traits color all history made by Tartars, or negroes, or Malays.

The climate which surrounds a tribe of men may affect the activities of these men as individuals or as an aggregate; education may intensify their powers or mellow their prejudices; oppression may make them servile or dominion make them overbearing, but these traits and their resultants, so far as science knows, do not 'run in the blood.' They are not 'bred in the bone.' Older than climate or training or experience are the traits of heredity, and in the long run it is always 'blood which tells.'

IV. On the other hand, the deeds of a race of men must in turn determine its blood. Could we with full knowledge sum up the events of the past history of any body of men, we could indicate the kinds of men destroyed in these events. The others would be left to write the history of the future. It is the 'man who is left' in the march of history who gives to history its future trend. By the 'man who is left' we mean simply the man who remains at home to become the father of the family—as distinguished from the man who in one way or another is sacrificed for the nation's weal or woe. If any class of men be destroyed by political or social forces, or by the action of institutions, they leave no offspring, and their like will cease to appear.

V. 'Send forth the best ye breed.' This is Kipling's cynical advice to a nation which happily can never follow it. But could it be accepted literally and completely, the nation in time would breed only second-rate men. By the sacrifice of their best, or the emigration of the best, and by such influences alone, have races fallen from first-rate to second-rate in the march of history.

VI. For a race of men or a herd of cattle are governed by the same laws of selection. Those who survive inherit the traits of their own actual ancestry. In the herd of cattle, to destroy the strongest bulls, the fairest cows, the most promising calves, is to allow those not strong, nor fair, nor promising, to become the parents of the coming herd. Under this influence the herd will deteriorate, although the individuals of the inferior herd are no worse than their own actual parents. Such a process is called race-degeneration, and it is the only race-degeneration known in the history of cattle or men. The scrawny, lean, infertile herd is the natural offspring of the same type of parents. On the other hand, if we sell or destroy the rough, lean, or feeble calves we shall have

a herd descended from the best. It is said that when the short-horned Durham cattle first attracted attention in England, the long-horns, which preceded them, inferior for beef or milk, vanished 'as if smitten by a pestilence.' The fact was that, being less valuable, their owners chose to destroy them rather than the finer Durhams. Thus the new stock came from the better Durham parentage. If conditions should ever be reversed, and the Durhams were chosen for destruction, then the long-horns might again appear, swelling in numbers as if by magic, unless all traces of the breed had in the meantime been annihilated.

VII. In selective breeding with any domesticated animal or plant, it is possible, with a little attention, to produce wonderful changes for the better. Almost anything may be accomplished with time and patience. To select for posterity those individuals which best meet our needs or please our fancy, and to destroy those with unfavorable qualities, is the function of artificial selection. Add to this the occasional crossing of unlike forms to promote new and desirable variations, and we have the whole secret of selective breeding. This process Youatt calls the 'magician's wand' by which man may summon up and bring into existence any form of animal or plant useful to him or pleasing to his fancy.

VIII. In the animal world progress comes mainly through selection, natural or artificial, the survival of the fittest to become the parent of the new generation. In the world of man similar causes produce similar results. The word progress is, however, used with a double meaning, including the advance of civilization, as well as race improvement. The first of these meanings is entirely distinct from the other. The results of training and education lie outside the scope of the present discussion. By training the force of the individual man is increased. Education gives him access to the accumulated stores of wisdom built up from the experience of ages. The trained man is placed in a class relatively higher than the one to which he would belong on the score of heredity alone. Heredity carries with it possibilities for effectiveness. Training makes these possibilities actual. Civilization has been defined as 'the sum total of those agencies and conditions by which a race may advance independently of heredity.' But while education and civilization may greatly change the life of individuals, and through them that of the nation, these influences are spent on the individual and the social system of which he is a part. So far as science knows, education and training play no part in heredity. The change in the blood which is the essence of race-progress, as distinguished from progress in civilization, finds its cause in selection only.

IX. To apply to nations the principles known to be valid in cattle-breeding, we may take a concrete example—that of the alleged decadence of France. It is claimed that the birth-rate is falling off in

France, that the stature is lower, and the physical force less among the French peasantry than it was a century ago. If all this is true, then the cause for it must be in some feature of the life of France which has changed the normal processes of selection.

X. In the present paper I shall not attempt to prove these statements. They rest, so far as I know, entirely on assertions of French writers, and statistics are not easily obtained. It suffices that an official commission has investigated the causes of reduced fertility, with chiefly negative results. It is not due primarily to intemperance nor vice nor prudence nor misdirected education, the rush to 'ready-made careers,' but to inherited deficiencies of the people themselves. It is not a matter of the cities alone, but of the whole body of French peasantry. Legoyt, in his study of 'the alleged degeneration of the French people,' tells us that "it will take long periods of peace and plenty before France can recover the tall statures mowed down in the wars of the republic and the First Empire," though how plenty can provide for the survival of the tallest this writer does not explain. Peace and plenty may preserve, but they can not restore.

It is claimed, on authority which I have failed to verify, that the French soldier of to-day is nearly two inches shorter than the soldier of a century ago. One of the most important of recent French books, by Edmond Demolins, asks, "in what consists the superiority of the Anglo-Saxon?" The answer is found in defects of training and of civic and personal ideals, but the real cause lies deeper than all this. Low ideals in education are developed by inferior men. Dr. Nordau and his school of exponents of 'hand-painted science' find France a nation of decadents, a condition due to the inherited strain of an overwrought civilization. With them the word 'degenerate' is found adequate to explain all eccentricities of French literature, art, politics, or jurisprudence.

XI. But science knows no such things as nerve-stress inheritance. If it did, the peasantry of France have not been subjected to it. Their life is hard, no doubt, but not stressful, and they suffer more from nerve-sluggishness than from any form of enforced psychical activity. The kind of degeneration Nordau pictures is not a matter of heredity. When not simply personal eccentricity, it is a phase of personal decay. It finds its causes in bad habits, bad training, bad morals, or in the desire to catch public attention for personal advantage. It has no permanence in the blood of the race. The presence on the Paris boulevards of a mob of crazy painters, maudlin musicians, drunken poets, and sensation-mongers proves nothing as to race degeneracy. When the fashion changes they will change also. Already the fad of 'strenuous life' is blowing them away. Any man of any race withers in an atmosphere of vice, absinthe and opium. The presence of such an at-

mosphere may be an effect of race decadence, but it is not a cause of the lowered tone of the nation.

Evil influences may kill the individual, but they can not tarnish the stream of heredity. The child of each generation is free-born so far as heredity goes, and the sins of the fathers are not visited upon him. If vice strikes deeply enough to wreck the man, it is likely to wreck or kill the child as well, not through heredity, but through lack of nutrition. The child depends on its parents for its early vitality, its constitutional strength, the momentum of its life, if we may use the term. For this a sound parentage demands a sound body. The unsound parentage yields the withered branches, the lineage which speedily comes to the end. But this class of influences, affecting not the germplasm, but general vitality, has no relation to hereditary qualities, so far as we know.

In heredity there can be no tendency downward or upward. Nature repeats, and that is all. From the actual parents actual qualities are received, the traits of the man or woman as they might have been, without regard, so far as we know, to the way in which these qualities have been actually developed.

XII. The evolution of a race is selective only, never collective. Collective evolution, the movement upward or downward of a people as a whole, irrespective of education or of selection, is, as Lepouge has pointed out, a thing unknown. 'It exists in rhetoric, not in truth nor in history.'

No race as a whole can be made up of 'degenerate sons of noble sires.' Where decadence exists, the noble sires have perished, either through evil influences, as in the slums of great cities, or else through the movements of history or the growth of institutions. If a nation sends forth the best it breeds to destruction, the second best will take their vacant places. The weak, the vicious, the unthrifty will propagate, and in default of better, will have the land to themselves.

XIII. We may now see the true significance of the 'Man of the Hoe,' as painted by Millet and as pictured in Edwin Markham's verse. This is the Norman peasant, low-browed, heavy-jawed, 'the brother of the ox,' gazing with lack-lustre eye on the things about him. To a certain extent, he is typical of the French peasantry. Every one who has traveled in France knows well his kind. If it should be that his kind is increasing, it is because his betters are not. It is not that his back is bent by centuries of toil. He was not born oppressed. Heredity carries over not oppression, but those qualities of mind and heart which invite or which defy oppression. The tyrant harms those only that he can reach. The new generation is free-born and slips from his hands, unless its traits be of the kind which demand new tyrants.

Millet's Man of the Hoe is not the product of oppression. He is

primitive, aboriginal. His lineage has always been that of the clown and swineherd. The heavy jaw and slanting forehead can be found in the oldest mounds and tombs of France. The skulls of Engis and Neanderthal were typical men of the hoe, and through the days of the Gauls and Romans the race was not extinct. The 'lords and masters of the earth' can prove an alibi when accused of the fashioning of the terrible shape of this primitive man. And men of this shape persist to-day in regions never invaded by our social or political tyranny, and their kind is older than any existing social order.

That he is 'chained to the wheel of labor' is the result, not the cause, of his impotence. In dealing with him, therefore, we are far from the 'labor problem' of to-day, far from the workman brutalized by machinery, and from all the wrongs of the poor set forth in the conventional literature of sympathy.

XIV. In our discussion of decadence we turn to France first simply as a convenient illustration. Her sins have not been greater than those of other lands, nor is the penalty more significant. Her case rises to our hand to illustrate a principle which applies to all human history and to all history of groups of animals and plants as well. Our picture, such as it is, we must paint with a broad brush, for we have no space for exceptions and qualifications, which, at the most, could only prove the rule. To weigh statistics is impossible, for the statistics we need have never been collected. The evil effects of 'military selection' and allied causes have been long recognized by students of social science, but their ideas have not penetrated into the common literature of common life.

The survival of the fittest in the struggle for existence is the primal cause of race progress and race changes. But in the red field of human history the natural process of selection is often reversed. The survival of the unfittest is the primal cause of the downfall of nations. Let us see in what ways this cause has operated in the history of France.

XV. First, we may consider the relation of the nobility to the peasantry, the second to the third estate.

The feudal nobility of each nation was in the beginning made up of the fair, the brave and the strong. By their courage and strength their men became the rulers of the people, and by the same token they chose the beauty of the realm to be their own.

In the polity of England this superiority was emphasized by the law of primogeniture. On 'inequality before the law' British polity has always rested. Men have tried to take a certain few to feed these on 'royal jelly,' as the young queen bee is fed, and thus to raise them to a higher class—distinct from all the workers. To take this leisure class out of the struggle and competition of life, so goes the theory, is to make of the first-born and his kind harmonious and perfect men and

women, fit to lead and control the social and political life of the state. In England, the eldest son is chosen for this purpose, a good arrangement, according to Samuel Johnson, 'because it ensures only one fool in the family.' For the theory of the leisure class forgets that men are made virile by effort and resistance, and the lord developed by the use of 'royal jelly' has rarely been distinguished by perfection of manhood.

The gain of primogeniture came in the fact that the younger sons and the daughters' sons were forced constantly back into the mass of the people. Among the people at large this stronger blood became the dominant strain. The Englishmen of to-day are the sons of the old nobility, and in the stress of natural selection they have crowded out the children of the swineherd and the slave. The evil of primogeniture has furnished its own antidote. It has begotten democracy. The younger sons in Cromwell's ranks asked on their battle-flags why the eldest should receive all and they nothing. Richard Rumbold, whom they slew in the Bloody Assizes, "could never believe that Providence had sent into the world a few men already booted and spurred, with countless millions already saddled and bridled for these few to ride." Thus these younger sons became the Roundhead, the Puritan, the Pilgrim. They swelled Cromwell's Army, they knelt at Marston Moor, they manned the Mayflower, and in each generation they have fought for liberty in England and in the United States. Studies in genealogy show that all this is literally true. All the old families in New England and Virginia trace their lines back to nobility, and thence to royalty. Almost every Anglo-American has, if he knew it, noble and royal blood in his veins. The Massachusetts farmer, whose fathers came from Plymouth in Devon, has as much of the blood of the Plantagenets, of William and of Alfred as flows in any royal veins in Europe. But his ancestral line passes through the working and fighting younger son, not through him who was first born to the purple. The persistence of the strong shows itself in the prevalence of the leading qualities of her dominant strains of blood, and it is well for England that her gentle blood flows in all her ranks and in all her classes. When we consider with Demolins 'what constitutes the superiority of the Anglo-Saxon,' we shall find his descent from the old nobility, 'Saxon and Norman and Dane,' not the least of its factors.

XVI. On the continent of Europe the law of primogeniture existed in less force, and the results were very distinct. All of noble blood were continuously noble. All belonged to the leisure class. All were held on the backs of a third estate, men of weaker heredity, beaten lower into the dust by the weight of an ever-increasing body of nobility. The blood of the strong rarely mingled with that of the clown. The noblemen were brought up in indolence and ineffectiveness. The evils of dissipation wasted their individual lives, while casting an ever-in-

creasing burden on the villager and on the 'farmer who must pay for all.'

XVII. Hence in France the burden of taxation led to the Revolution and its Reign of Terror. I need not go over the details of dissipation, intrigue, extortion and vengeance which brought to sacrifice the 'best that the nation could bring.' In spite of their lust and cruelty, the victims of the Reign of Terror were literally the best from the standpoint of race development. Their weaknesses were those of training in luxury and irresponsible power. These effects were individual only, and their children were free-born, with the capacity to grow up truly noble if removed from the evil surroundings of the palace.

XVIII. In Thackeray's 'Chronicle of the Drum,' the old drummer, Pierre, tells us that

> "Those glorious days of September
>   Saw many aristocrats fall,
> 'Twas then that our pikes drank the blood
>   In the beautiful breast of Lamballe.
>
> "Pardi, 'twas a beautiful lady,
>   I seldom have looked on her like,
> And I drummed for a gallant procession
>   That marched with her head on a pike."

Then they showed her pale face to the Queen, who fell fainting, and the mob called for her head and the head of the King. And the slaughter went on until the man on horseback came, and the mob, 'alive but most reluctant,' was itself forced into the graves it had dug for others.

And since that day the 'best that the nation could bring' have been without descendants, the men less manly than the sons of the Girondins would have been, the women less beautiful than the daughters of Lamballe. The political changes which arose may have been for the better; the change in the blood was all for the worse.

XIX. Other influences which destroyed the best were social repression, religious intolerance and the intolerance of irreligion and unscience. It was the atheist mob of Paris which destroyed Lavoisier, with the sneer that the new republic of reason had no use for savants. The old conservatism burned the heretic at the stake, banished the Huguenot, destroyed the lover of freedom, silenced the agitator. Its intolerance gave Cuvier and Agassiz to Switzerland, sent the Le Contes to America, the Jouberts to Holland, and furnished the backbone of the fierce democracy of the Transvaal. While not all agitators are sane, and not all heretics right-minded, yet no nation can spare from its numbers those men who think for themselves and those who act for themselves. It cannot afford to drive away or destroy those who are filled with religious zeal, nor those whose religious zeal takes a form not approved by tradition nor by consent of the masses. All

movements toward social and religious reform are signs of individual initiative and individual force. The country which stamps out individuality will soon live in the mass alone.

XX. A French writer has claimed that the decay of religious spirit in France is connected with the growth of religious orders of which celibacy is a prominent feature. If religious men and women leave no descendants, their own spirit, at least, will fail of inheritance. A people careless of religion inherit this trait from equally careless ancestors.

XXI. Indiscriminate charity has been a fruitful cause of the survival of the unfit. To kill the strong and to feed the weak is to provide for a progeny of weakness. It is a French writer again, who says that "Charity creates the misery she tries to relieve; she can never relieve half the misery she creates."

There is to-day in Aosta, in Northern Italy, an asylum for the care and culture of idiots. The crétin and the goitre are assembled there, and the marriage of those who can not take care of themselves ensures the preservation of their strains of unfitness. By caring devotedly for those who in the stress of life could not live alone for a week and by caring for their children, generation after generation, the good people of Aosta have produced a new breed of men, who can not even feed themselves. These are incompetent through selection of degradation, while the 'man of the hoe' is primitively ineffective.

The growth of the goitre in the valleys of Savoy, Piedmont and Valais is itself in large part a matter of selection. The boy with the goitre is exempt from military service. He remains at home to become the father of the family. It is said that at one time the government of Savoy furnished the children of that region with lozenges of iodine, which were supposed to check the abnormal swelling or the thyroid gland, known as the goitre. This disease is a frequent cause of idiocy or cretinism, as well as its almost constant accompaniment. It is said the mothers gave the lozenges only to the girls, preferring that the boys should grow up to the goitre rather than to the army. The causes of goitre are obscure, perhaps depending on poor nutrition, or on mineral substances in the water. The disease itself is not hereditary so far as known, but susceptibility to it certainly is. By taking away for outside service those who are resistant, the heredity of tendency to goitrous swelling is fastened on those who remain.

Like these mothers in Savoy was a mother in Germany. Not long since, a friend of the writer, passing through a Franconian forest, found a young man lying senseless by the way. It was a young recruit for the army who had got into some trouble with his comrades. They had beaten him and left him lying with a broken head. Carried to his home, his mother fell on her knees and thanked God, for this injury had saved him from the army.

XXII. The effect of alcoholic drink on race progress should be considered in this collection. Authorities do not agree as to the final result of alcohol in race selection. Doubtless, in the long run, the drunkard will be eliminated, and perhaps certain authors are right in regarding this as a gain to the race. On the other hand, there is great force in Dr. Amos G. Warner's remark, that of all caustics gangrene is the most expensive. The people of southern Europe are relatively temperate. They have used wine for centuries, and it is thought by Archdall Reid and others that the cause of their temperance is to be found in this long use of alcoholic beverages. All those with vitiated or uncontrollable appetites have been destroyed in the long experience with wine, leaving only those with normal tastes and normal ability of resistance. The free use of wine is, therefore, in this view, a cause of final temperance, while intemperance rages only among those races which have not long known alcohol, and have not become by selection resistant to it. The savage races which have never known alcohol are even less resistant, and are soonest destroyed by it.

In all this there must be a certain element of truth. The view, however, ignores the evil effect on the nervous system of long-continued poisoning, even if the poison be only in moderate amounts. The temperate Italian, with his daily semi-saturation is no more a normal man than the Scotch farmer with his occasional sprees. The nerve disturbance which wine effects is an evil, whether carried to excess in regularity or irregularity. We know too little of its final result on the race to give certainty to our speculations. It is moreover true that most excess in the use of alcohol is not due to primitive appetite. It is drink which causes appetite, and not appetite which seeks for drink. In a given number of drunkards but a very few become such through inborn appetite. It is influence of bad example, lack of courage, false idea of manliness, or some defect in character or misfortune in environment which leads to the first steps in drunkenness. The taste once established takes care of itself. In earlier times, when the nature of alcohol was unknown and total abstinence was undreamed of, it was the strong, the boisterous, the energetic, the apostle of 'the strenuous life,' who carried all these things to excess. The wassail bowl, the bumper of ale, the flagon of wine, all these were the attribute of the strong. We can not say that those who sank in alcoholism thereby illustrated the survival of the fittest. Who can say that as the Latin races became temperate they did not also become docile and weak? In other words, considering the influence of alcohol alone, unchecked by an educated conscience, we must admit that it is the strong and vigorous, not the weak and perverted, that are destroyed by it. At the best, we can only say that alcoholic selection is a complex force, which makes for temperance—if at all, at a fearful cost of life which without alcoholic temp-

tation would be well worth saving. We cannot easily, with Mr. Reid, regard alcohol as an instrument of race-purification, nor believe that the growth of abstinence and prohibition only prepares the race for a future deeper plunge into dissipation. If France, through wine, has grown temperate, she has grown tame. "New Mirabeaus," Carlyle tells us, "one hears not of; the wild kindred has gone out with this, its greatest." This fact, whatever the cause, is typical of great, strong, turbulent men who led the wild life of Mirabeau because they knew nothing better.

XXIII. The concentration of the energies of France in the one great city of Paris is again a potent agency in the impoverishment of the blood of the rural districts. All great cities are destroyers of life. Scarcely one would hold its own in population or power were it not for the young men of the farms. In such destruction Paris has ever taken the lead. The education of the middle classes in France is almost exclusively a preparation for public life. To be an official in a great city is an almost universal ideal. This ideal but few attain, and the lives of the rest are largely wasted. Not only the would-be official, but artist, poet, musician, physician or journalist seeks his career in Paris. A few may find it. The others, discouraged by hopeless effort or vitiated by corrosion, faint and fall. Every night some few of these cast themselves into the Seine. Every morning they are brought to the morgue behind the old Church of Notre Dame. It is a long procession and a sad one from the provincial village to the strife and pitfalls of the great city, from hope and joy to absinthe and the morgue. With all its pitiful aspects the one which concerns us is the steady drain on the life-blood of the nation: its steady lowering of the average of the parent stock of the future.

XXIV. But far more potent for evil to the race than all these influences, large and small, is the one great destroyer—War. War for glory, war for gain, war for dominion, its effect is the same whatever its alleged purpose.

SCIENTIFIC LITERATURE.

ETHICS AS A SCIENCE.

Thanks to such writers as Spencer, Stephen and Sutherland, we have been long familiar with ethics treated from a scientific standpoint. Yet the science of ethics, as pursued by these thinkers, betrayed one evident defect—it proceeded by analogy from the physical sciences. In the new work, entitled 'Ethics, Descriptive and Explanatory' (Macmillan), by Professor Mezes, of the University of Texas, an effort is made to remove this reproach. His aim "is to give as adequate critical and methodical an account as possible of what morality and immorality *are* . . . to construct a positive or purely scientific theory of Ethics, and to give a naturalistic account of all the aspects of morality and immorality." Mr. Mezes does not forget that this is a vast undertaking, one not to be compassed within the limits of a text-book such as this professes to be. But, remembering these restrictions, we may say that he has produced an excellent work; indeed, so excellent, that it were well worth his while to consider whether it might not be wise for him to view it as the prospectus of a far more ambitious undertaking, in which some, if not all, the major problems could be wrought out with fullness. The plan pursued by Mr. Mezes is as follows: In the Introduction, he defines ethics, shows its scope and method, and distinguishes between moral and non-moral phenomena. The body of the book consists of two parts, the first dealing with subjective morality and the individual conscience; the second discussing objective morality, and embracing, among other inquiries, an admirable analysis of justice. A conclusion treats the nature and value of morality. As the work is undoubtedly of considerable importance, several interesting features deserve mention. Mr. Mezes is thoroughly objective in his method, and so approaches, within his chosen sphere, the standpoint which a biologist might occupy in his. Significant in this connection is his shrewd suggestion that ethics is not to be treated as a teleological science till you come to the end of it. He is to be commended greatly, further, for the even-handed way in which he grapples with the ticklish questions of conscience and the like. He shows clearly that *Moralitat*, while by no means of the importance assigned it by the traditional English and theological moralists, cannot be overlooked. In particular, he contrives to put the results of psychological research to good use in his analysis. This is one of several pleasing and hopeful features. Similarly, in this connection, he rids himself of the time-honored static conception of conscience, and, by adopting a dynamic theory, actually vindicates a concrete place in moral life for this hoary abstraction. So, too, when he passes to objective morality *(Sittlichkeit)*, and makes contact with the cardinal virtues. Under his sober hand, these cease to be vague entities floating in mid-air, and come to take their places as vital results of objective morality—results shot out, as it were, by the interaction of man with man. The chapter on justice deserves to rank with the best discussions of the subject. Mr. Mezes, in short, has managed to free himself from many of the stultifications that have beset scientific moralists in the past. Whether he has emancipated himself from all need not be discussed now. It is sufficient to note that he has produced a fresh, suggestive and most careful work; that he has adopted and held fast to a scientific

standpoint in ethics—not in biology or psychology or any other science, and that, therefore, he has advanced the cause of objective research in this most baffling field. A few books of this character and the present inextricable tangle in ethical theory might be in a fair way toward ravelling up.

BOTANICAL BOOKS.

Dr. D. H. Scott has rewritten a series of lectures given at the University College, London, 1896, and published them under the title of 'Studies in Fossil Botany' (A. & C. Black). This book will be a most useful one to the botanist, since it presupposes no knowledge of paleontology, and discusses only the portions of a subject of major importance to the student of plants. A perusal of this work will impress the reader with the enormous amount of light thrown on the natural affinities of plants by the results of paleobotanical investigations during the last ten or twelve years.

'Elements de paleobotanique' (Carré & Naud), by R. Zeiller, is a comprehensive text-book, in which the entire subject receives a thorough and systematic treatment. The preservation of fossils, classification and nomenclature, systematic examination of the principal types of fossil vegetation, floral succession, climate, etc., are among the principal topics taken up at length. The bibliographic list in the appendix covers eighteen pages and is inclusive of the greater number of important titles.

Professor Percival, of Southeastern Agricultural College, Kent, England, has written a text-book of 'Agricultural Botany' (Duckworth & Co.), which will meet the needs of students interested in plants from a cultural point of view more nearly than any similar text-book hitherto published. The eight chief divisions of the book are concerned with the general external morphology of the plant, internal morphology, physiology, classification, and special botany of farm crops, weeds, farm seeds and fungi, considered chiefly in relation to some of the common diseases of plants and bacteria. The matter is arranged in two portions; a didactic discussion of the principles of the subject, which has been kept as free as might be from technicalities, and a series of demonstrations and experiments, by which all the more important points are actually seen in the plant. The point of view throughout the entire book is entirely different from that of the lecturer on pure botany, and the perspective of the entire subject is rearranged to meet the new conditions. It is impossible, of course, that all the more important recent discoveries, even in such a basal portion of the work as the nutrition of plants, should be put into practise immediately, but it is to be said that Professor Percival's book is fairly abreast of the times, although adhering to some anachronisms. The introduction and use of the book in America would be followed by a notable improvement of the instruction in botany in most agricultural schools.

'The New Forestry' (Pawson & Brailsford, Sheffield), by Mr. John Simpson, is a manual adapted to British woodlands and game preservation. One chapter is devoted to the management of a woodland as a place for sheltering and rearing pheasants and other game birds and animals. The remaining chapters are devoted to practical directions as to rotation, allotment, cultural methods and general administration of forests, with a consideration of the numerous factors that must be taken into account in forestry operations on an English estate. The practical value of the book is enhanced by estimates of expenses and selling values.

THE BEET SUGAR INDUSTRY.

The report on the 'Progress of the Beet-Sugar Industry in the United States in 1899' presents a very hopeful outlook for the success of this industry over a quite wide range of territory. The report was prepared by the Department

of Agriculture on the basis of extensive observations in the field and at beet-sugar factories, and chemical examination of beets grown at a large number of places in forty-one States and Territories. Experiments to determine the regions best adapted to profitable beet culture have been in progress for several years past, and in connection with similar work conducted by the State experiment stations, have in large measure settled this question. On the basis of the results, over 30 beet-sugar factories have been established and are in successful operation. A number of others are now building, and still others are in contemplation, if contracts can be made with farmers for growing the beets. California has eight factories, including the largest factory in the world, with a capacity for working 3,000 tons of sugar beets per day, which is an indication of the energy with which this new industry is starting in America. It was expected that 35,000 acres of beets would be grown for this factory in 1900. Nine factories were in operation in Michigan, where for several reasons the conditions are considered particularly favorable to the industry, and the greatest interest has been manifested in its development. An interesting feature of the factory at Lehi, Utah, is the establishment of a slicing station or subfactory at a point thirty miles away, where the juice is extracted from the beets, limed and piped to the main factory. Another subfactory in an opposite direction is planned, increasing the capacity of the combined plant to 1.200 tons of beets a day. This plan of having 'slicing stations' connected with the main factory by pipe lines is a novel one, and is believed to be a distinct advancement. It saves expense in hauling the beets and brings a larger radius of farming country into close contact with the sugar factory. The factory at Carlsbad, New Mexico, is said to be the only factory in the world where sugar beets are grown entirely with irrigation. Its demonstration of the feasibility of this is considered a valuable lesson for the arid regions. The average cost of raising an acre of sugar beets, under conditions similar to those in Iowa, for example, is given as $30, and the yield at from twelve to fifteen tons, although under extraordinary conditions it may reach twenty-five tons. The price paid for beets by the factories depends in many cases on the sugar content, but averages about $4 to $4.50 per ton. In many localities where the conditions are favorable it has been demonstrated to the satisfaction of the farmer that a larger profit can be realized from growing sugar beets than any other crop, and in addition the land is improved by the superior cultivation given this crop. Furthermore, the value of the extracted sugar-beet pulp as a feeding stuff for animals is urged as an additional advantage to the agriculture in the vicinity of beet-sugar factories, which is being appreciated. This pulp is usually given away for the hauling, but in some cases the factories themselves have erected feeding pens, where large numbers of cattle and sheep have been fattened. Time and effort have been required to induce farmers to take up the growing of beets on account of the large amount of labor and the expense involved, and many expensive lessons have had to be learned in the operation of factories; but the industry is now believed to be well on its feet, with a good prospect of steady growth.

THE PROGRESS OF SCIENCE.

There appears to be no abatement in expeditions for polar discovery and adventure. Lieutenant Peary remains in the far north, seeking to reach a point nearer to the Pole than did Dr. Nansen and the Duke of Abruzzi's party, while with the same object in view Mr. Baldwin is preparing an expedition, liberally equipped by Mr. Ziegler, of New York City, and Captain Bernier is making efforts to secure a similar outfit in Canada. These expeditions are perhaps not primarily for scientific research, though they should add to knowledge in many directions. The expeditions being fitted out with the assistance of the German and British Governments for antarctic exploration are, however, strictly scientific in character. Exploration in the north has never relaxed, but since Sir James Ross returned, in 1843, efforts to explore the south polar region have been sporadic and comparatively unimportant, until the recent expeditions under Captain de Gerlache and Mr. Borchgrevink. The scientific results of these expeditions have not yet been published, though descriptive volumes by Mr. Borchgrevink and Dr. Cook have recently been issued, and the latter has contributed to the present number of this Journal an interesting account of the unknown southern aurora. The 'Belgica,' from which Dr. Cook made his observations, was not, however, altogether fortunate in its course, and possibly the dramatic interest of the first antarctic night is greater than the scientific interest of the results. Mr. Borchgrevink followed pretty closely in the track of Sir James Ross, and his own book contributes little or nothing to scientific knowledge. He reached by a day's expedition a point furthest to the south, but it is not even obvious how he determined this, when he estimates the semi-diameter of the sun as 16° 17′ 1″. However valuable the scientific results of the voyages of the 'Belgica' and of the 'Southern Cross' may prove when published, there is certainly room for the great expeditions now being made ready in England and in Germany.

The 'Discovery,' which will carry the British Antarctic Expedition, was launched on March 21 from the yard of the Dundee Shipbuilders' Company. No fewer than six ships with this name have been engaged in British exploration, and the present vessel is somewhat similar to its namesake, which took part in Sir George Nares's expedition in 1875. But it, of course, contains all modern improvements, and is of unusual strength. The oak ribs are placed as close together as possible. These are covered on the outside with oak and greenheart and on the inside with asbestos, while the bow is cased with steel plates. The tonnage is 1,750, the length at the water line 172 feet, and the extreme breadth 33 feet. The engines are of 450 horse power, giving a speed of about eight knots an hour, but to save coal they will be sparingly used, the vessel being rigged as a bark with three masts. Great care has been taken with the interior fittings to secure the greatest possible efficiency of scientific work, with due regard to the comfort of the company. The vessel is under the command of Capt. Robert Scott, and Prof. J. W. Gregory, who has recently gone from the British Museum to Melbourne University, is in charge of the scientific work. The expedition will begin its work at Victoria Land, facing New Zealand, where Ross and, recently, Mr. Borchgrevink, have

explored furthest to the south. The German expedition, under Dr. von Drygalski, is also making active preparation, and its vessel—which has been named 'Gauss,' in honor of the great mathematician—was launched on April 1. Expeditions to cooperate with those from England and Germany are also planned in Scotland and Sweden. It seems unfortunate that the United States, which sixty years ago, at the time of the great antarctic expeditions by Ross, d'Urville and Balleny, sent Wilkes with five vessels, should not be represented in the present movement to make a thorough exploration of the antarctic regions.

While Great Britain is sending out its antarctic expedition at a cost of $500,000, a less pretentious, but perhaps equally interesting expedition is being planned. In view of the enormous importance attached to the recent discoveries of the relation of mosquitoes to malaria, and perhaps to yellow fever, Dr. Patrick Manson has urged the sending of a party to the islands of the Pacific, and, in the first instance, to Samoa, to study the life history of the mosquito and the conditions on which its existence and development depend. In certain of the islands of the Pacific, elephantiasis, a disease also due to the mosquito, is so prevalent that it occurs in half or more of the population, while in other islands it is entirely absent. It is hoped that the study of the distribution of mosquitoes, and, perhaps, experiments on their introduction, may show what is antagonistic to their development, thus making it possible to find a means of destroying them when they are present. Towards this plan the sum of $2,500 has been subscribed anonymously, and it is hoped that the British Government will assist in providing the $10,000 necessary to carry it into effect. It seems evident that the Department of Agriculture should at once undertake the study of the distribution of the malaria-bearing mosquitoes in the United States. The annual money loss to the country through the prevalence of malaria may be as little as $10,000,000 or as much as $100,000,000, but it is in any case so enormous that a thorough investigation, at whatever cost, would be in the direction of the strictest economy. There are, for example, no *Anopheles* on Manhattan Island, but within a mile of it they are abundant and malaria is prevalent. It may be supposed that the value of real estate, at the seashore and mountain resorts, for example, will be doubled or halved, according as *Anopheles* are absent or present.

The plague has now been so long prevalent in India that the newspapers no longer regard it as necessary to report on it, and probably very few think of its ravages, yet the deaths in Bengal alone during the last week, of which reports are at hand, were 4,000, and the recent census of India shows that the population of Bombay is 50,000 less than before the epidemic. The occurrence of the plague at Cape Town has, however, attracted notice, in view of the possibility of its spreading in the British Army, and attention has recently been called to the existence of the disease in San Francisco. It has for a long time been known in medical circles that there have been cases of plague in the Chinese quarters, but the State authorities have denied their existence and have attempted to suppress any information in regard to the epidemic. It appears that Secretary Gage appointed some time since, in spite of the protest of the Governor of California, a commission to investigate the matter. This commission, consisting of Prof. Simon Flexner, of the University of Pennsylvania; Prof. F. G. Novy, of the University of Michigan, and Prof. L. F. Barker, of the University of Chicago, has made a thorough investigation and has presented a report, from which it appears that thirty-two fatal cases have occurred in San Francisco during the past year; and this probably is incomplete, as six deaths were

discovered by the commission referred to above in the course of a single week, and no cases have been reported that were not fatal. The State has now been aroused, and has appropriated $100,000 for the Board of Health to use in the suppression of the epidemic. One branch of the Legislature passed a most extraordinary bill, making it a felony to publish, by writing or printing, that Asiatic cholera or bubonic plague exists within the State, unless the fact has been determined by the State Board of Health and entered upon its minutes, but this measure appears now to have been dropped. The San Francisco papers have apparently been only too ready to suppress information in regard to the plague in that city. It appears that the epidemic is slight, but it will naturally be exaggerated by attempts to deny its existence for commercial reasons.

Within the past six months the attention of the English public has been attracted in an unwonted degree to the question of the purity of alcoholic liquors. There occurred last fall, in Lancashire, and especially in Manchester and its vicinity, large numbers of cases of arsenical poisoning, which were finally traced to the consumption of a particular brand of beer. Further investigation revealed the fact that the manufacturers of this beer used, in brewing, glucose of a certain make, and that the manufacturers of this glucose had recently begun to use in its preparation a sulfuric acid which was made from pyrites containing, as is almost invariably the case, arsenic. Prior to this time it appears that the sulfuric acid used had been that made from sulfur. It was a long chain of evidence, but was complete, for arsenic was found in the beer, in the glucose, in the acid and in the pyrites, and the amount found in the beer corresponded to that in the ingredients used in its manufacture. The quantity was amply sufficient to occasion all the symptoms of poisoning which were noticed. Several points of interest have been brought out in the voluminous discussions which have followed this incident, or tragedy, as it would be better to call it. In the first place, attention has been called to the difficulty of detecting arsenic in beer and similar liquids by methods which had been commonly used. In this way several analysts were led to pronounce beer to be free from arsenic, which was afterwards shown by other methods to contain notable quantities of the poison. It now appears that the test most to be relied on in such cases is that of Reinsch, which consists essentially in boiling the beer, strongly acidified with pure hydrochloric acid, with clean copper foil, and then subliming the black deposit obtained on the copper, if arsenic is present, in a glass tube. The presence of a sublimate of bright octahedral crystals of arsenious oxid is certain evidence of arsenic in the beer. Difficulties in carrying out the ordinary tests for arsenic with many beers, which were examined in large numbers when the public had been aroused to the danger of contaminated beer, led to the discovery of substances added to the beer, which had no legitimate place in brewing, and which bid fair to occasion a much closer supervision of this industry in the future. Attention has been called also to other industries where sulfuric acid is used, and where arsenic which may be present would be carried over into products destined for general consumption. This is especially true in the case of many substances used in pharmacy. It has also been shown that inasmuch as sulfur is always accompanied by small quantities of the rare element selenium, it is not impossible that its compounds, which are very poisonous, may often be present in sufficient quantity to exert a deleterious influence.

This subject has been given a somewhat different turn by the work of Sir Lauder Brunton and Dr. Tunnicliffe upon the injurious constituents of distilled liquors. It is now nearly a score

of years since the remarkable experiments of Dujardin-Beaumetz on the toxic action of the different alcohols. He found that the toxic action of pure ethyl alcohol (common alcohol) was in a certain sense *nil*, that is to say, hogs which were kept in a condition of intoxication most of the time for nearly three years, on being allowed to sober up, appeared to be in perfect health, and presented after slaughtering no visible lesions of any organ. This was the case when absolutely pure liquor was used, but when ordinary spirits were fed to hogs they quickly succumbed, showing symptoms and lesions, especially of the liver, similar to those only too familiar in the case of human inebriates. The conclusion, drawn by Dujardin-Beaumetz from a long series of experiments, was that the toxic quality of alcoholic liquors is due chiefly to the presence of higher alcohols, especially amyl alcohol, the principal ingredient of fusel oil, though methyl alcohol and aldehyde may play a subordinate part. Under any circumstances no distilled liquor is safe to use till it has been 'aged' for several years in the wood. Brunton's researches, on the other hand, seem to show that the presence of fusel oil, in such quantities as it usually occurs in potable liquors, is not a menace to public health, but that the greatest danger is from the presence of furfural and other similar aldehydes, which are derived from the husk of the grain under the influence of heat and acids. Furfural is present to a greater or less extent in all whiskies, but is especially abundant in those made by modern processes, where it is sought to obtain as much liquor as possible per bushel of grain. According to this, the superiority of the liquors of 'ye olden time' was due not so much to the fact that they were better 'aged,' but because they originally contained less of the furfural, having been made more carefully. Brunton's physiological experiments were exceedingly interesting, especially in comparing the after effects of intoxication from ordinary spirits with those of spirits from which the furfural had been removed. In the latter case as soon as the animal was sober it appeared to be in a perfectly normal condition, and showed none of the after effects, which in the former case lasted for a considerable time. It is also worthy of note that those substances popularly used as 'bracers' after intoxication generally contain ammonia or some allied compound, which, from a chemical standpoint, is capable of combining with the furfural and neutralizing its effects.

Since the comparatively recent condensation of hydrogen to a liquid, much study has been devoted to its physical properties, and especially to the determination of its boiling-point, since this is not far above the absolute zero. The difficulty regarding the former determinations, which gave the boiling point as —238.4° C., is that being obtained by means of a platinum resistance thermometer, they depended upon extrapolation, which might prove faulty at such low temperatures, as has now indeed been shown to be the case. More recently Dewar has made use of a constant-volume gas thermometer, employing for the gas hydrogen from different sources, and also helium, contaminated with only slight traces of neon. The results obtained show that the boiling-point of hydrogen is —252.5°, or 20° above the absolute zero. Investigations as to the temperature of solid hydrogen are now being carried out, and show a still closer approach to the absolute zero. For some years the researches of Gautier in Paris have indicated that hydrogen is a normal constituent of the atmosphere, and the question may now be considered as settled. Not only has Dewar condensed hydrogen directly from the atmosphere, but Gautier has made quantitative determinations of the amount in different localities. In the air of Paris hydrogen does not seem to be an invariable constituent, though methane (marsh gas) is always present and traces of carbon monoxid, while the unsaturated hydrocarbons are generally

absent. In forest air traces of hydrogen were present, and about half as much methane as in the air of Paris. At a mountain station in the Pyrenees at an elevation of 2,785 meters only two volumes of methane per 100,000 were found, but seventeen volumes of hydrogen. At a sea station, 40 kilometers from the coast of Brittany, only traces of methane were found, but nearly two volumes of hydrogen in 10,000, an amount two-thirds as great as that of carbon dioxid. The source and fate of atmospheric hydrogen is a problem which now awaits solution. Liveing and Dewar seem of the opinion that there is a continual accession of hydrogen to the atmosphere from interplanetary space, and Stoney holds that the earth's gravitational attraction is insufficient to retain hydrogen in the atmosphere. Experiments of Gautier show that when certain crystalline rocks are heated with water a considerable quantity of hydrogen is evolved, which might cause a constant accession of the gas to the atmosphere. The problem must be considered for the present unsolved.

Professor Nipher, of Washington University, St. Louis, has discovered that the most sensitive photographic plates may be manipulated in open daylight, and perfect pictures may be developed upon them in sunlight instead of in the dark room. The pictures are separately wrapped in black paper in the dark room, and boxed. They may then be separately unwrapped, in the open fields if necessary, and placed in the plate holders. The camera exposure must be very much greater than in the dark room methods. After the exposure, the plate is taken out into the light and placed in the developing solution. Even if direct sunlight falls upon the plate for a moment during these changes, fine pictures may be developed. There is, however, no advantage in unnecessarily exposing the plate. The developing bath may always be in shadow, but beautiful pictures have been developed in direct sunlight. The pictures produced in this way are positives, while those produced in the dark room by ordinary methods are negatives. The positive is the picture ordinarily obtained by printing off from the negative. The shadows show light on the negative and dark on the positive. The positives produced in this way are greatly superior to those produced in the dark room on over-exposed plates, and the exposure time is very much less, but may be very great. Such pictures of a crowded street show the street with perfect clearness, every moving thing being eliminated. In one exposure lasting for several hours, a team which had stood in one position for half an hour showed no trace upon the plate when developed.

Every one who has had experience in photography has lost valuable plates by over-exposure. But Professor Nipher shows that all exposures may be successfully developed. Exposures ranging from a snapshot to an over-exposure of about 2,000 may be developed in the dark room as negatives. The fogging in over-exposed plates is an approach to a zero condition, where the plate is blank. For such exposures bromide is freely used, and a few drops of saturated hypo are added. In ordinary dark room work hypo is carefully avoided. But as the zero condition is approached, it is very useful in keeping the plate clear. As soon as the exposure is so great that the plate cannot be controlled in the dark room, it may be developed in the light. Plates a million times over-exposed can be thus developed. The amount of illumination of the plate while being developed depends upon the amount of exposure in the camera. Instead of using the camera, the plate can be exposed in a printing frame, where it takes the place of the sensitive paper. An exposure of two or three minutes, just out of direct sunlight at a south window, may be developed in the same light. The best results are obtained with a hydrochinone developer. Some photographic

plates have given poor results in daylight, and Professor Nipher recommends Cramer's 'crown' plate.

The new star in Perseus, which has now waned in the sky, and in the memory of most people, is still an object of discussion among astronomers. Our readers will remember Professor Newcomb's recent article on variable stars and the difficulties in the way of accounting for their periodicity. In the extreme case of new stars the difficulty is greatest. The theories of an outburst from the molten interior and of collision might account for the appearance of the star, but do not explain its rapid waning, nor are they in accord with spectroscopic determinations. Professor Seeliger's theory that a dark star passes through a swarm of meteors is the most satisfactory form of hypotheses, but leaves room for the ingenious suggestion, recently made by the great astronomer, M. Janssen, before the Paris Academy of Sciences. He points out that the apparent absence of oxygen from the sun may be due to its existence in some dissociated condition that the spectroscope would not reveal. This condition may be owing to a very high temperature, and when this becomes low enough to allow oxygen to assume its common form, and so to unite with hydrogen, there would ensue, as a result of the combustion, a great increase in heat and light, which would account for the brilliancy of a new star. The rapid decrease in brilliancy which follows would be accounted for by the formation of an atmosphere of vapor, which would serve as a gradually increasing obstacle to radiation from the star. A corollary of M. Janssen's supposition is that our own sun may at any time reach this transition point for oxygen and blaze out into a fury of heat and light that would scorch all life off the face of the earth. It is, however, a pleasant feature of solar catastrophes that astronomical time is measured by millions of years.

One of the most interesting total eclipses of the sun, which the present century furnishes, will occur on May 18, 1901. The maximum duration of totality, which will be about six and a half minutes, is rarely surpassed. This will give exceptional opportunity, provided the sky is clear, for work of any kind, photographic or visual. The region of totality is, however, inconveniently remote, and the weather conditions, which usually prevail at the stations which will be occupied, are not of the best. The shadow begins off the east coast of Africa, a short distance to the southwest of Madagascar, sweeps northeasterly over the Indian Ocean, and crosses Central Sumatra, Southern Borneo and New Guinea, and a few smaller islands. To visit the track of the eclipse from New York, therefore, one must journey half way around the earth, and it matters little, so far as distance is concerned, whether one starts east or west. In spite of the distance, observations will be undertaken by a number of American and European astronomers. In this country, the Yerkes, Lick, Columbia, Amherst and Naval Observatories and the Massachusetts Institute of Technology will be represented by skilled observers. Under the auspices of the Royal and Royal Astronomical Societies, English observers will be stationed at Mauritius, and Padang, on the west coast of Sumatra. At Padang the eclipsed sun will be only 21° from the zenith, and the duration of totality about six and a half minutes. At Mauritius, the chances for a clear sky are much greater than at any other station, but the duration of the total phase is only three and a half minutes. On this account, nearly all the American and European observers are planning to visit Sumatra. The observations may have an added value from the fact that the eclipse occurs near the time of minimum sun-spot activity.

The chief part of the work in this, as in other recent eclipses, will doubtless be photographic. Owing to the enormous advantages which photo-

graphic methods of research give, they should undoubtedly be extensively employed, but it may be hoped that visual observations by skilled observers will not be neglected. There is a tendency in certain directions to regard solar eclipses as of less importance than formerly. This may be due, in part, to the fact that investigations, which in the past could only be carried on at times of total eclipse, can now be studied throughout the year, and, in part, to the very large number of observations which have already been made. Eclipse expeditions, also, are very expensive, and often end in total failure, owing to clouds. Photography has multiplied the results many times in recent years, but for the solution of many problems in solar physics, as complete records as possible for a long time are necessary. In spectroscopic lines it seems hardly possible to obtain too much material for some time to come. Perhaps more of mystery and interest attaches to the corona than to any other feature, and the present eclipse gives an excellent opportunity for several lines of investigation, in addition to photographs showing its structure and extent. An attempt will again be made to investigate the rotations of the corona, by photographs of its spectrum, which must be sufficiently good to show the slight displacement of the lines caused by the motion of rotation. It is to be hoped, also, that further bolometric observations will be made on the heat radiations of the corona, as well as a study of the polarization of the coronal light. Aside from the sun itself, the existence or non-existence of an intra-mercurial planet has not been clearly demonstrated, since investigations in that line up to the present time have not been conclusive. Certainly no amount of time and labor can be regarded too great, which may be necessary to give us as complete a mastery as possible of the problems which relate to our great parent, the sun.

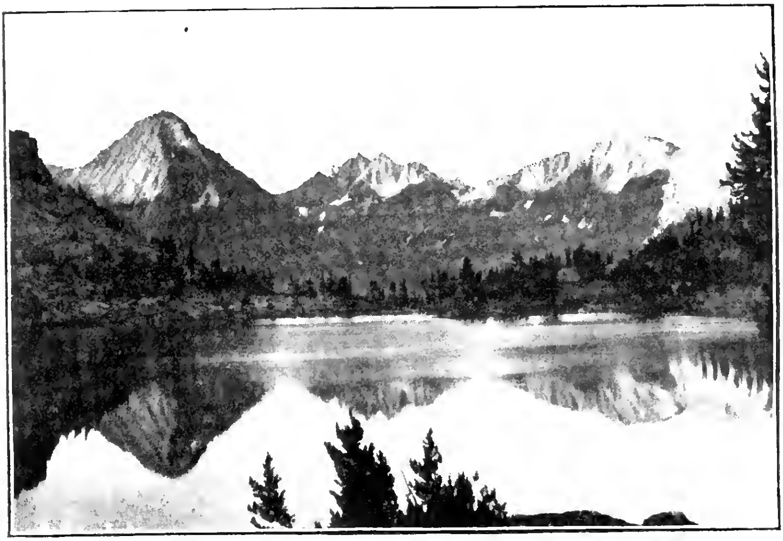
Yale and Princeton, the two most conservative of our larger universities, have recently taken action that will bring their college courses more into harmony with those of other leading institutions, by giving greater opportunity to elect scientific in the place of classical studies. At Yale, Greek and Latin are still required through the freshman year, but later these studies are elective. In the sophomore year five or six courses must be elected from twelve that are offered, making it possible for a student to specialize in science. In the junior and senior years, the chief work of the student may also lie in the sciences, unhampered by restrictions other than that he must take two courses in languages and literature and two courses in philosophy, history and social science. Courses can also be elected, as at Columbia and Pennsylvania, which count as part of the medical course. At Princeton, President Patton has made somewhat similar proposals looking towards offering courses in physiology and human anatomy, so that students may begin their medical education in the senior year. He, at the same time, suggested adding to the electives in the sophomore year. At present Princeton University requires Latin, Greek and the Bible through the freshman and sophomore years, while about one-third of the student's time is occupied with required studies in the junior year.

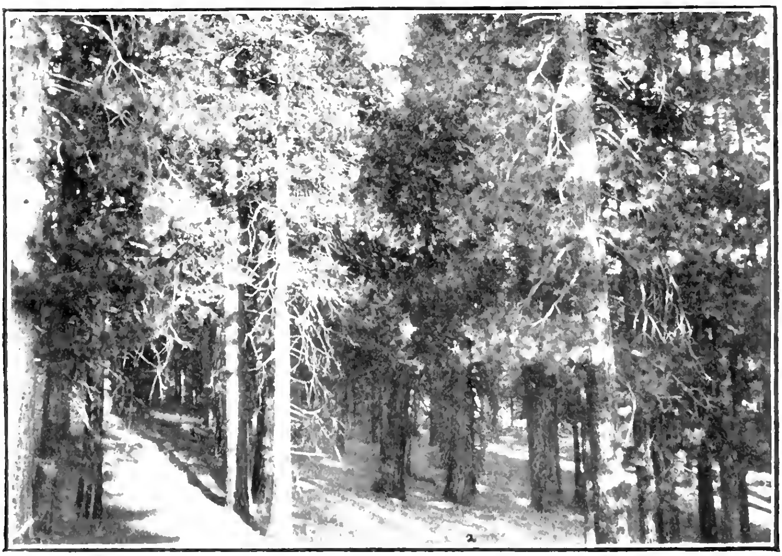
Cornell now admits students to its B. A. course without Latin, and Harvard requires no Latin at the University, but still maintains an entrance examination. Columbia requires Latin in the freshman year, but has recently made it possible for a student to enter without Latin, though he cannot graduate until he has studied this language. The great universities of the Middle and Western States have in most cases established three degrees—A. B. for those who pass entrance examinations in Latin and Greek and study these languages to a greater or less degree in their college course; B. Ph. for those

who do not study Greek, and B. S. for those who study neither Latin nor Greek. It has resulted that only a small proportion of students has taken the A. B. degree, yet the other degrees referred to have no definite and well-established meaning. The bachelor of science degree, for example, does not mean that a student has had a scientific education, but simply that he has not studied Latin and Greek. Under these circumstances it appears that the Universities of Michigan and Minnesota have during the past month taken a forward step in abolishing all college degrees except the A. B., giving this for all courses of liberal studies. It is obvious that the A. B. no longer means a classical education when both in England and the United States its only condition is 'small Latin' in the preparatory school. Scientific students might like to see a degree established that definitely signifies a scientific education —as the B. Sc. of the University of London. The authorities of Columbia University recently considered the desirability of offering such a degree, but it was thought impossible to give the B. S. a definite signification.

Dr. George Davidson, professor of geography in the University of California, has been elected a correspondent of the Paris Academy of Sciences.—St. Andrews University has conferred its LL. D. on Mr. Alexander Agassiz, of Harvard University, and Aberdeen University has conferred the same honor on Professor Rudolf Virchow, of Berlin.—Mr. J. J. H. Teall, F. R. S., has been appointed director-general of the Geological Survey of Great Britain and Ireland, in succession to Sir Archibald Geikie, who retired on February 28. Sir Archibald has been in the service of the Survey for forty-six years and has reached the age limit.—Prof. S. M. Babcock, of the University of Wisconsin, inventor of the Babcock milk test, was, on March 27, presented with a medal, voted him by the State for giving his invention free to the world. Exercises were held in the Assembly Chamber of the Capitol in the presence of both Houses of the Legislature, the university faculty and regents and many prominent citizens of the State. Governor Lafollete presided, and addresses were made by him, by ex-Governor W. D. Hoard and others.—A committee has been formed to erect at Heidelberg a monument in memory of three of its great scientific men, Bunsen, Kirchoff and von Helmholtz.—A memorial marble bust of Robert Brown, the eminent botanist, formerly a student at Aberdeen, presented to the university by Miss Hope Paton, has been unveiled in the picture gallery of Marischal College.—Three expert geologists from the United States Geological Survey (Dr. C. Willard Hayes, Mr. T. Wayland Vaughan and Mr. A. C. Spencer) have been detailed to make a geologic and mineral reconnaissance of the Island of Cuba.—The Coast and Geodetic Survey steamships, Pathfinder and McArthur, at San Francisco, and the Patterson and Gedney, at Seattle, are now fitting up, under orders to proceed to Alaska to survey important passages among the islands along the Alaskan cost.—Dr. Patrick Geddes, who was responsible for the formation of the International Association for the Advancement of Science, Arts and Education, and the holding of an International Assembly at the Paris Exposition, last year, proposes a similar assembly, in connection with the exposition and congresses to be held at Glasgow this year. —The second Latin-American Scientific Congress opened its two-weeks' session at Montevideo on March 20, with over 200 delegates in attendance. Dr. Robert Wernicke, professor of pathology in the University of Buenos Aires, Argentine Republic, was elected president of the Congress.—In order to make the free distribution of seeds by the United States Department of Agriculture as useful as possible, Secretary Wilson has secured authority to send out young trees as well as seeds.

The death of Dr. William Jay Youmans is a personal loss not only to his many friends, but also to the thousands of those who knew him only as editor of this journal. Youmans was born near Saratoga on October 14, 1838, and the boyhood on his father's farm gave him the training which has so often led to the elevation of public and professional life in this country. He was descended, as his name witnesses, from the British Yeomanry, and the sterling stock that settled in New England was typified in his person and character. He loved his home in the country, and had purchased a farm nearby, to which it was his intention to retire to pass the years of rest that he had so well earned. After leaving the home farm at the age of seventeen, Youmans studied under his brother, the late Dr. E. L. Youmans, and later at Yale, Columbia and New York Universities, and in London under Huxley. He practised medicine for several years in Minnesota, and in 1872 joined his brother in New York to establish the Popular Science Monthly. For twenty-eight years his life was devoted to this journal, first in association with his brother—who was seventeen years the older, and died in 1887—and afterwards as editor-in-chief. The two brothers not only edited the journal, but as advisers of the house of Appleton, gave them their high standing as publishers of scientific books in the renaissance of science based on the doctrine of evolution. The teachings of Spencer, Darwin and other great leaders were for them a religion to which their lives were consecrated. Their influence through this journal and other publications of the Appletons was great and permanent. Youmans died at Mount Vernon on April 10 from typhoid fever, after a ten days' illness. His life was devoted with rare singleness of purpose to the diffusion of science; it was a privilege to know him; he was gentle, kind and noble.





STANISLAUS RESERVATION.

THE POPULAR SCIENCE MONTHLY.

JUNE, 1901.

OUR FOREST RESERVATIONS.

By Professor J. W. TOUMEY,
YALE FOREST SCHOOL.

It is highly probable that the future will chronicle the act of March 3, 1891, under which the Chief Executive of the United States is given power to segregate forest reservations from the public domain, as a law most fruitful in results of vast import to the future welfare of the country. Armed with the power conferred by this act, the successive Presidents have in the past ten years established no less than thirty-nine national forest reservations.

As the act provides that the reservations are to be segregated from the public domain, they are for the most part in the Rocky Mountain region and in the Pacific Coast States where large areas of public forest lands were available.

The thirty-nine reservations in the aggregate contain more than 46,800,000 acres, an area more than fifteen times as large as the State of Connecticut, or about one-fortieth of the total area of the country exclusive of Alaska.

Much controversy has arisen as to the wisdom of withdrawing such large areas of the public lands from sale or from other disposition under the laws of the land office. Much of the opposition has disappeared during the past few years, and public sentiment in favor of forest reservations is rapidly increasing. In fact, so rapid has been this change in public sentiment that a movement is now on foot, with prospect of success, to establish a national forest reservation in the southern Appalachian Mountains, where it will be necessary for the Government to purchase the land at an expense of several million dollars. There is also an effort being made on the part of a good many public-

spirited citizens to establish a national reservation in Minnesota, at the head of the Mississippi River. Unfortunately, however, the latter effort is at present checked by the lumber interests of the region, although these interests would profit in the long run by the establishment of the reservation.

Forest reservations are not entirely national affairs. Stae reservations are already an established fact in a few States and the indications are that they will be formed in many others during the next decade. The State forests in the Adirondack Mountains in the State of New York are splendid examples of such reservations. These lands were purchased at State expense that they might remain forever in forest, a great heritage for both pleasure and profit for all time.

Similar reservations have been established during the past few years in Pennsylvania, and others are likely to be set aside in Michigan before the close of the present year.

Going hand in hand with the making of the State and National reservations, there has been a rapid development in public sentiment as to the importance of practical forestry and its application to the management of the wooded areas of the country, both public and private.

This change in public sentiment is well illustrated in the volume and character of the investigations in forestry by the Government, when compared with what they were a few years ago. In the Division of Forestry of the Department of Agriculture alone, the appropriations have increased more than six-fold in three years, thus making it possible to extend the study of important problems in American forestry to many of the varied sections of the country. It is well illustrated in the rapidly increasing facilities for instruction in technical forestry in our recently established forest schools and the courses in forestry offered in many of our colleges and universities. It is shown in the fact that owners of private woodlands are in some instances employing trained foresters to superintend their lumbering operations, so that their methods of cutting will not interfere with the perpetuation of the forest. It is shown in the yearly increasing appropriations for forestry investigations by the legislatures of the several States, but most of all it is shown in the rapidly increasing number of applications coming to the trained foresters of the Government from the owners of private woodlands for assistance and advice in the management of their forests and in establishing plantations of forest trees.

I desire to make clear that this changing sentiment regarding our forests is most fortunate for our future welfare. American prosperity has been largely due to the productiveness of American soil, *i. e.*, to her agricultural and forest products, the value of the latter approximating $1,000,000,000 per year at the present time. The effect

upon the soil of these two classes of products is very different. Agricultural crops being removed when mature, practically in their entirety, impoverish the soil, while forest crops, being removed only in part and then at long intervals of time, have an opposite effect, as they for the most part enrich the soil.

For many reasons it is highly important that even in agricultural regions a varying proportion of the land should remain in forest, not only for the direct value of the products which it affords and its value in enriching the soil, but for its beneficial influence upon the adjacent cultivated fields which it is not necessary for me to recount here.

THE FOREST RESERVATIONS ARE STILL THE HAUNTS OF THE RAREST AND LARGEST GAME THAT THE COUNTRY AFFORDS. STANISLAUS RESERVATION, CALIFORNIA.

If it be desirable that a certain proportion of our agricultural lands be kept as woodland, it is important that they be made to produce desirable products in the largest degree consistent with economy. This can only be brought about by a rational system of management, where skill and foresight is exercised to as great a degree as in the successful production of agricultural crops.

Although much might be said regarding the importance of well-managed woodland in agricultural regions, it is to the vast area of non-agricultural land in this country that the application of practical forestry will be of incalculable value. It is highly important that our non-agricultural lands be made to contribute toward our national

wealth. There is no other contribution which they are capable of making that will compare, both directly and indirectly, with their forest growth.

Experience has abundantly shown that the natural selfishness of man leads him to excesses in the utilization of forest products. His tendency is not only to consume a product equal to the growth of his own time, but to make large inroads upon the future. He is profligate in the use of wood, often leaving all but the very best to decay upon the ground or to become fuel for forest fires.

The justification for our forest reservations should not, however, be based entirely upon their value in conserving timber. They have,

EXCESSIVE, UNRESTRICTED AND INDISCRIMINATE GRAZING HAS INVARIABLY LED TO THE DESTRUCTION OF THE YOUNG GROWTH ON THE FOREST FLOOR. BLACK MESA RESERVATION, ARIZONA.

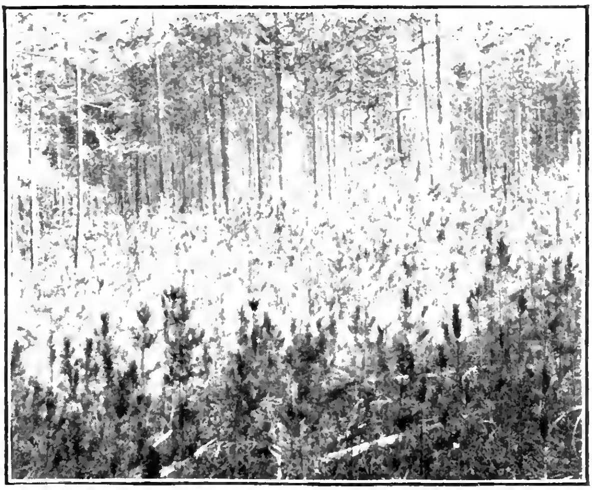
for the most part, been wisely selected to fulfil a threefold function, viz.: that of protection and luxury, as well as that represented in the direct value of forest products. Indeed, at the present time their direct value is in many instances of minor importance. On the other hand, as the reserved lands are almost entirely mountainous in character and located at the headwaters of many of our important streams, their value as conservators of moisture is very great, and it is to their maintenance in many instances that the farmers and ranchmen in the adjacent valleys must look for a perennial supply of water for their crops and stock.

In the selection of the reservations, consideration has also been given to their value from the standpoint of recreation and sport.

They contain a large part of the wildest, grandest and most picturesque portions of the American continent, and many of them are still the haunts of the rarest and largest game that the country affords.

The segregation of the forest reservations from the public lands, without the establishment and execution of regulations for their protection and management, would have but little effect in itself upon the preservation of their forests as shown in the present condition of the forests on our unreserved lands. Excessive, unrestricted and indiscriminate grazing has invariably led to the destruction of the young growth on the floor of the forest.

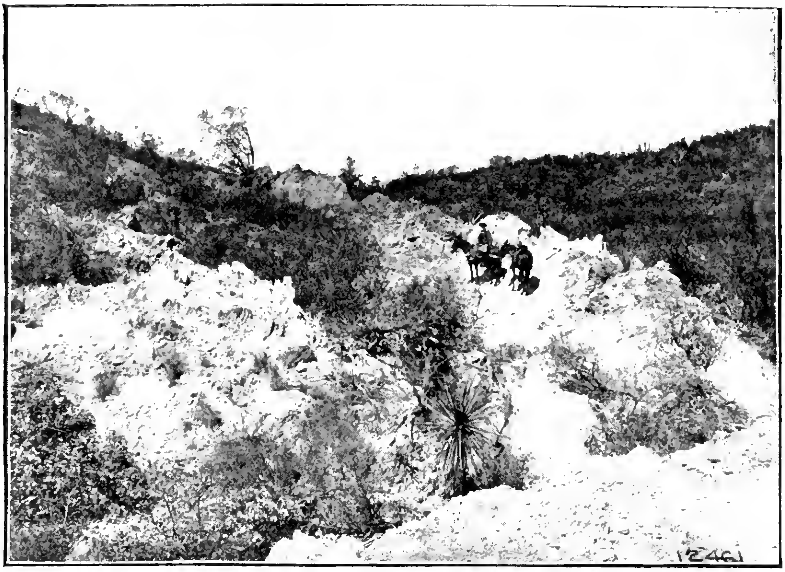
Where such grazing is continued for a number of years, the forest



YOUNG PINE SEEDLINGS—THE FUTURE FOREST—WHERE UNINJURED BY FIRE AND GRAZING. OLYMPIC RESERVATION, WASHINGTON.

rapidly deteriorates, for there are not a sufficient number of young trees to form a proper leaf canopy when the old ones are removed or when they mature and decay. We appear to lack a realizing sense that it is the young growth and not the old trees that insure the perpetuation of the forest.

As the reservations could not be treated in similar manner as the unreserved lands, with any expectation of preserving or improving the forest growth, provision was made by the U. S. Land Office, which was responsible for the management of the reservations, for the appointment of certain forest officials, viz.: superintendents, supervisors and forest rangers, these officers having immediate control of the reserved lands as to management and protection. Largely from their lack of both



The Cover of the Southwestern Reservation is mostly Chaparral, either mixed with a Scattered Growth of Single Trees or Wholly of Shrubby Plants. San Jacinto Reservation, California.

The Altitude is often too great for the Growth of Desirable Timber. Sierra Reservation, California.

a practical and technical knowledge of forestry, and, in most cases, even of woodcraft, their work has been necessarily limited. From lack of training and experience they have been unable to create and put into execution a practical system of forest management for the lands under their control. Although unable to cope with the problems of management, they have been able in many instances to afford the reservations a fair degree of protection from fire and grazing.

As the forests of the reservations must eventually be utilized for their timber and other forest products, in order to make direct contributions to the national wealth, the work of management must go beyond that of simply protecting them from fire and grazing, even if this were afforded to the fullest degree possible.

They should be so managed that wherever the mature timber has material value it can be harvested and sold. The utilization of the forest products, however, must not interfere with the perpetuation of the forest. The cutting must be so conducted that the forest be maintained in the best possible condition as to reproduction and growth consistent with economy. In order to do this it is necessary that the reservations be under the control of practical and trained foresters.

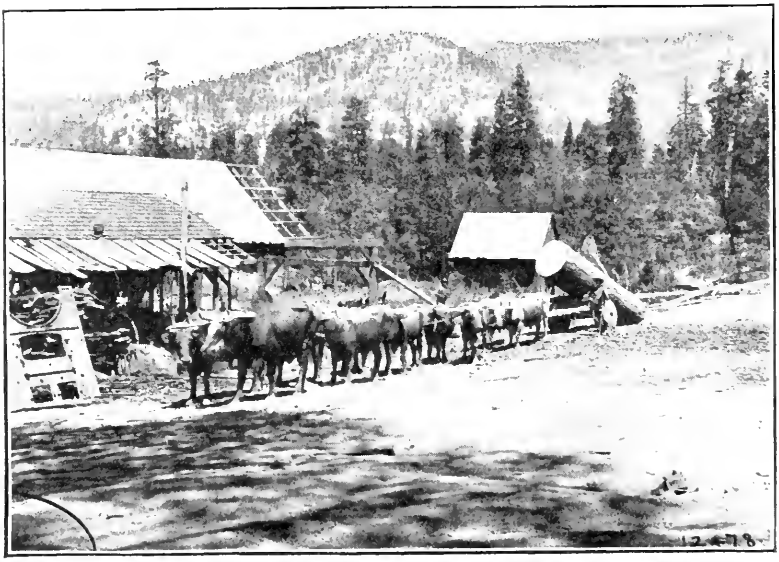
It is extremely gratifying to know that within the past few months the direct management of the national reservations, so far as it relates to questions of practical and economic forestry, has been transferred to the Division of Forestry of the Department of Agriculture, where they will receive attention from trained foresters. Working plans will be made for all the reservations, and the prospects are extremely flattering that on these 46,800,000 acres of reserved forest lands there will develop a system of American forestry that will have far-reaching influence on our future prosperity.

At first thought it may appear that it is not necessary to make forest reservations for the purpose of conserving the timber and lesser forest products in a country so splendidly wooded as the United States. When we consider, however, that, from the most reliable sources of information that we have, the amount of timber consumed exceeds the amount normally produced by the forests, we must know that the excess of consumption is at the expense of the main wood capital. In many instances this decrease is not so much on account of decrease in area as on account of decrease in productive capacity of the forests themselves.

Having such a splendid and large original supply to draw upon, we consume much more wood per capita than any other nation. At our present rate of consumption the most reliable authority that we have places the present supply as sufficient for our requirements for about fifty years, without taking into consideration the annual increment of the forests during this period.

It is difficult for us to comprehend our present yearly consumption of wood in all its varied uses. Conservative and accepted authorities place the present yearly lumber cut in this country at 40,000,000,000 feet (B. M.), while this is estimated to be but one-seventh of the total wood consumption. If it were possible to cut the entire amount yearly consumed into boards an inch thick, they would cover a walk six feet wide that would extend more than 354 times around the earth at its greatest diameter.

Although the amount of wood produced each year by the growth of the forests of the entire country is very great, it is a long way from what it might be both in quantity and quality were our forests ade-



Timber grows but to be utilized. Lumbering on Private Holdings within the Boundaries of the San Jacinto Reservation, California.

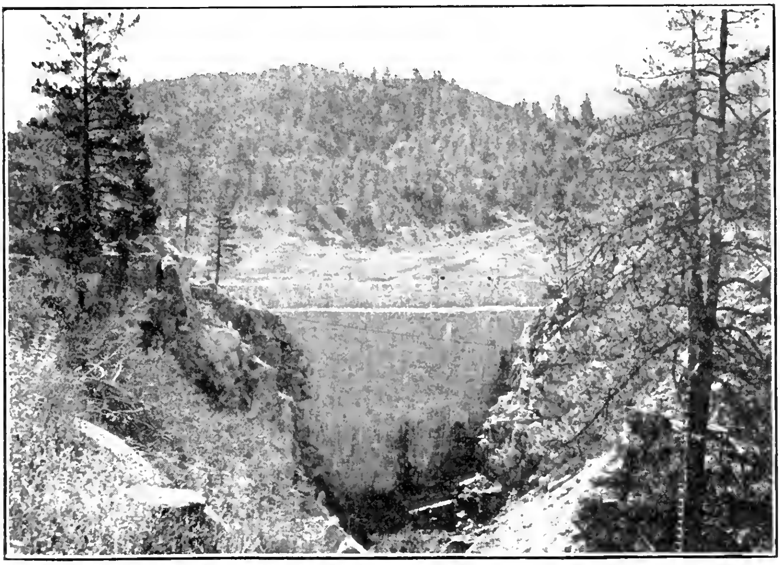
quately protected and managed. It will certainly not be sufficient to supply our requirements, after the virgin timber is exhausted, without the organization of a system of management which will keep the lands assigned to forest growth properly protected and in a desirable condition as to reproduction and growth.

This is well illustrated in the present unsatisfactory condition of much of the woodland in the Eastern States that has been cut over at various times without consideration for a future crop and left without protection and to chance reproduction. In the oldest part of the Union, viz.: the original thirteen States, the latest report, based upon trustworthy figures, places the wooded area at a little over fifty-five per cent., yet without systematic forest management, how

utterly inadequate this comparatively large area is to provide those States with their present wood requirements, more particularly timber of desirable dimensions for first-class lumber.

As one would naturally expect from the great variations in climate and topography, there are marked differences in the reservations in the quality and quantity of timber per acre. Indeed, such reservations as the San Jacinto and San Gabriel in Southern California, reserved primarily for the protection which they afford the adjacent cultivated lands, bear merchantable timber, but on a small percentage of their total area.

The large part of the vegetation is brush or chaparral, either mixed



THE HEMIT RESERVOIR, SAN JACINTO RESERVATION, CALIFORNIA.

with a scattered stand of single trees or wholly composed of shrubby plants. It should hardly be dignified by the term forest.

From these Southwestern reservations of the arid and semi-arid regions, with little timber of commercial importance, to the rich stands of splendid timber, covering large areas of the Washington and Mount Ranier reservations in the State of Washington, our thirty-nine reservations show all variations in the density of their forests. On the whole, however, but few of them have a large percentage of their total area covered with first-class commercial trees. In some instances the altitude is too great for the growth of desirable timber, while in others the lack of moisture will not permit its growth at low elevations. It is in the intermediate zones that tree growth is at its best.

Although a large number of species make up the forests of the reservations, they are for the most part composed of pines and other conifers, with the yellow pine and red fir a long way in the lead in commercial importance. The former of these two species is found in every one of the thirty-nine reservations, with the exception of the Apognak Reservation, in Alaska, and in many of them forms the major part of the forest, while the latter has nearly as wide a distribution.

In the Washington reservation pure stands of red fir may be classed among the finest forests in the world. Not infrequently single trees reach a height of from 250 to 300 feet, and contain 25,000 feet (B.M.) of merchantable lumber. The trees stand close together, their long, straight boles shooting upward like so many shafts from the dimly-lighted bed of moss and ferns forming the floor of the forest. This

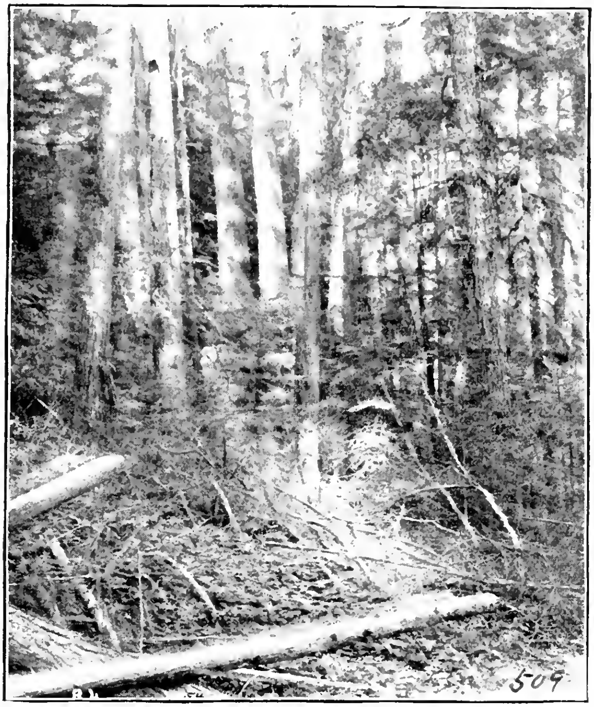
Our Reservations must be protected from Fire, so far as an Efficient Forest Service can protect them. Olympic Reservation, Washington.

same tree, of a more stunted and shorter growth, forms a considerable part of the forests of the more southern reservations, even growing in the forests of Arizona. Here, however, the forest is open and the drooping limbs cover the boles nearly to the ground, rendering them of little value for commercial purposes, but of vast importance in shading the ground and thus aiding in the conservation of moisture.

No greater mistake can be made than to consider the timber supply of the reservations as confined to the mature trees that we find growing there at the present time. We should look into the future and ask what are these 46,800,000 acres of reserved lands capable of producing as an annual increment when properly protected and managed. What kind of forests are they capable of producing in the future, long after the trees now living shall have been harvested or have gone to decay?

The value of this vast inheritance, which is placed in our keeping for future generations, will depend upon how well we manage it. By this is not meant how well we protect the mature trees from the woodman's axe, but how well we protect the tender seedlings, that are to form the future forest, from being destroyed from outside influences.

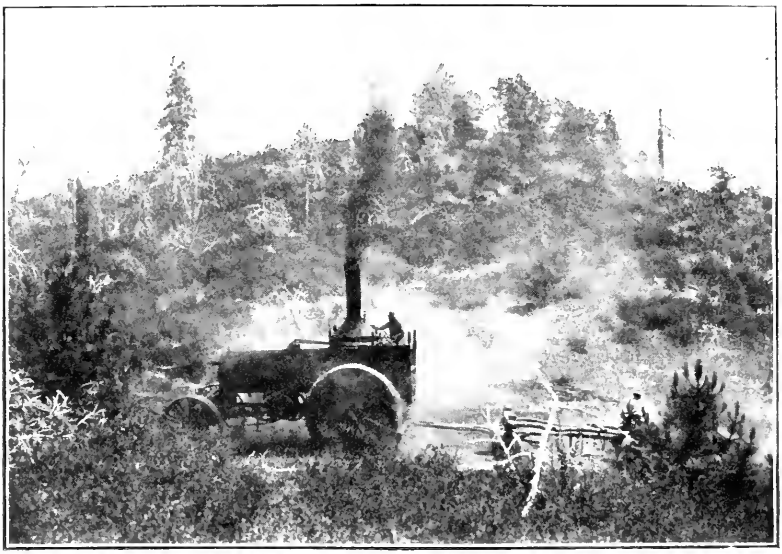
Timber is grown but to be utilized, hence it is the duty of those having the reservations in charge to see that it is utilized at the proper time wherever accessible and of sufficient value to pay for the cutting.



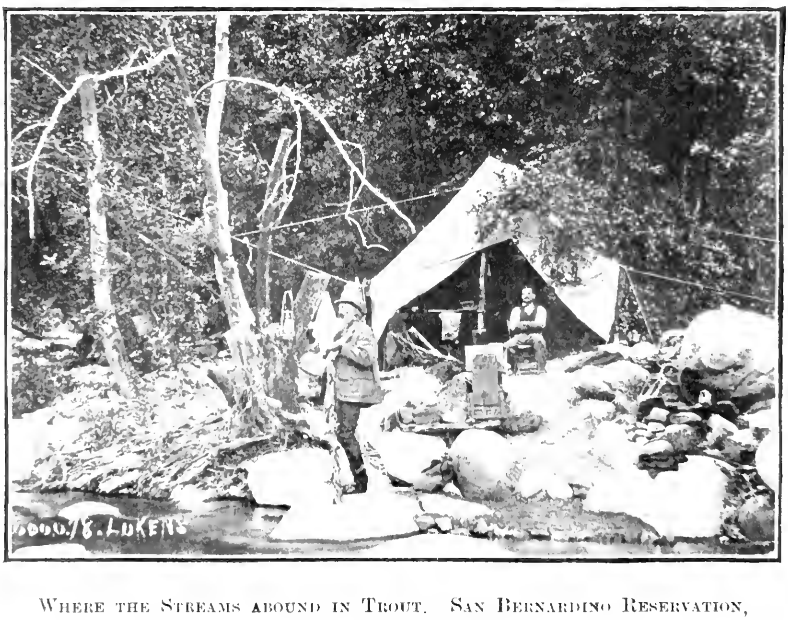
FORESTS OF SAME REGION AS SHOWN IN PREVIOUS ILLUSTRATION WHERE UNINJURED BY FIRE. OLYMPIC RESERVATION, WASHINGTON.

It is far more important, however, at the present time, to preserve and improve every factor that leads toward the perpetuation of the forest and in keeping it at its best in reproduction and growth.

It is worth while to consider briefly the indirect value of the forest reservations from the standpoint of water conservation. Although this is a factor to be taken into account in considering the value to the nation of each of the reservations, nowhere is it more apparent than in Arizona and Southern California, where the scarcity of water and its utilization for purposes of irrigation give it enormous value. It is to



THE CONDUCTING OF LOGGING OPERATIONS ON PRIVATE LANDS WITHIN THE RESERVATIONS CAUSES MANY FIRES, SOME OF WHICH ESCAPE TO THE RESERVED LANDS. SAN BERNARDINO RESERVATION, CALIFORNIA.



WHERE THE STREAMS ABOUND IN TROUT. SAN BERNARDINO RESERVATION, CALIFORNIA.

a large measure the reservations of these regions and the preservation of their forest cover that give such great value to the adjacent cultivated fields. It is the water and not the land that has value. It is the perennial supply, flowing from the reserved and unreserved forests of East and Central Arizona, that has in the past two decades rescued the Salt River Valley from its former barrenness, with its scattered growth of creosote brush and cacti, and transformed it into one of the most fertile and productive areas in America. It is the forest cover of the San Jacinto and San Barnardino reservations in Southern California that gave Riverside and Redlands her splendid orange groves and made possible the development of a productive and thriving community.

When in our Western forests one is constantly impressed by the change in relative humidity wrought wherever the forest has been removed. Springs have disappeared and cañons and ravines are now dry, where there were formerly perennial streams. Under the leaf mold and other débris of the forest, the soil is always moist, while on denuded areas in the same locality it is parched and dry. Everywhere the deep mulch forming the floor of the forest grasps the descending rains and melting snows and guides them into the deeper recesses of the earth. Where the forests have been destroyed, or even the mulch and litter forming the forest floor, as it so often is by fire or the excessive grazing of sheep, the rains for the most part, instead of sinking into the soil, pass over the surface, carrying silt and other débris into the streams and reservoirs, causing vital injury to irrigation enterprises.

So also in the semi-arid regions, where there are no forests, or where they have been destroyed, the wind has a free sweep, resulting in an enormous increase in evaporation. In some instances the evaporation from a water surface exposed to the free sweep of the wind reaches a maximum of thirteen inches in a single month. In exposed situations, snows a foot in depth are frequently lapped up in a single day without even moistening the soil beneath. We do not appreciate how great the necessity for the preservation of the forests is to the irrigable West.

Reservoirs for the purpose of impounding water to be used in irrigation have been constructed by private enterprise in many parts of the West, and the possibility of governmental construction of such reservoirs is by no means improbable. Effective reservoirs are not possible in our irrigable regions without due regard for the forests that feed the streams which fill them. Forests everywhere are the great preventors of erosion, and nowhere is this more evident than in our Western mountains. The utility of reservoirs, and, to a lesser extent, of distributing canals and laterals, becomes destroyed as they fill with

silt. To prevent this filling, the forests must be preserved; they must be protected from fire, in so far as an efficient forest service can protect them, and also from grazing, wherever it seriously interferes with the effectiveness of the forest floor as a water absorbent. In some of the Southwestern reservations, notably in Arizona, sheep-grazing has been carried so far that natural reproduction is at a standstill, and the forest floor has been made in some places almost as bare and compact as a road-bed. It is reasonable to expect that overgrazing will continue, until every hoof that enters the reservations is there under a permit based upon the judgment of a competent forester, who shall have absolute decision as to the portions of the forests that can be safely grazed and those that cannot.

One of the most fertile causes of injury to the forest cover of the reservations arises from the numerous private holdings of non-agricultural lands within their boundaries. From personal experience I know that the harvesting of the timber on these small areas of private lands in the San Bernardino reservation and the leasing of them for grazing purposes have been harmful to the reservation to a marked degree. The conducting of logging operations during the dry season by means of traction engines or by donkey engines and cables have caused numerous fires, some of which have escaped and burned over large areas of the reservation.

In driving sheep to the leased lands within the boundaries of the reservation, they have been grazed for months on the reserved lands, the leasing of the private holdings being primarily an excuse to get the stock within the reservation. It would seem desirable, therefore, that all such holdings be acquired by the Government, in order to eliminate the constant danger arising from them.

We should not overlook the value of the forest reservations as great national parks for recreation and sport, where those so inclined can go and get in touch with nature at her best; where the streams abound in trout, and wild animals are not confined behind iron bars; where there are no signs, 'Keep off the grass,' and, best of all, where one can build himself anew from wholesome mountain air and water, vigorous exercise and plain food.

With so much to commend both State and National reservations and with such vast areas of public lands at the command of the Government, it is somewhat surprising that their realization remained until the last decade of the nineteenth century. At last the forest has gained the resepct due it as a great economic and civilizing factor and is taking its true place in the esteem of all classes of public-spirited citizens.

THE BLOOD OF THE NATION.

A STUDY OF THE DECAY OF RACES THROUGH THE SURVIVAL OF THE UNFIT.—II. IN WAR

BY DAVID STARR JORDAN,
PRESIDENT OF LELAND STANFORD JUNIOR UNIVERSITY.

XV. Not long ago I visited the town of Novara, in northern Italy. There, in a wheatfield, the farmers have plowed up skulls of men till they have piled up a pyramid ten or twelve feet high. Over this pyramid some one has built a canopy to keep off the rain. These were the skulls of young men of Savoy, Sardinia and Austria—men of eighteen to thirty-five years of age, without physical blemish so far as may be, peasants from the farms and workmen from the shops, who met at Novara to kill each other over a matter in which they had very little concern. Should the Prince of Savoy sit on his unstable throne or yield it to some one else, this was the question. It matters not the decision. History doubtless records it, as she does many matters of less moment. But this fact concerns us—here in thousands they died. Farther on, Frenchmen, Austrians and Italians fell together at Magenta, in the same cause. You know the color that we call Magenta, the hue of the blood that flowed out under the olive trees. Go over Italy as you will, there is scarcely a spot not crimsoned by the blood of France, scarcely a railway station without its pile of French skulls. You can trace them across to Egypt, to the foot of the Pyramids. You will find them in Germany—at Jena and Leipzig, at Lützen and Bautzen and Austerlitz. You will find them in Russia, at Moscow; in Belgium, at Waterloo. 'A boy can stop a bullet, as well as a man,' said Napoleon; and with the rest are the skulls and bones of boys, 'ere evening to be trodden like the grass.' 'Born to be food for powder' was the grim epigram of the day, summing up the life of the French peasant. Read the dreary record of the glory of France, the slaughter at Waterloo, the wretched failure of Moscow, the miserable deeds of Sedan, the waste of Algiers, the poison of Madagascar, the crimes of Indo-China, the hideous results of barrack vice and its entail of disease and sterility, and you will understand the 'Man with the Hoe.' The man who is left, the man whom glory cannot use, becomes the father of the future men of France. As the long-horn cattle reappear in a neglected or abused herd of Durhams, so comes forth the aboriginal man, 'the man of the hoe,' in a wasted race of men.

XXVI. A recent French cartoon pictures the peasant of a hundred years ago plowing in a field, a gilded marquis on his back, tapping his gilded snuff-box. Another cartoon shows the French peasant of to-day, still at the plow. On his back is an armed soldier who should be at another plow, while on the back of the soldier rides the second burden of Shylock the money-lender, more cruel and more heavy even than the dainty marquis of the old régime. So long as war remains, the burden of France cannot be shifted.

XXVII. In the loss of war we count not alone the man who falls or whose life is tainted with disease. There is more than one in the man's life. The bullet that pierces his heart goes to the heart of at least one other. For each soldier has a sweetheart, and the best of these die, too—so far as the race is concerned—if they remain single for his sake.

In the old Scottish ballad of the 'Flower of the Forest' this thought is set forth:

> "I've heard the lilting at each ewe-milking
> Lassies a-lilting before the dawn of day.
> But now they are moaning, on ilka green loaning,
> For the 'Flower of the Forest' is a' wed away."

Ruskin once said that 'War is the foundation of all high virtues and faculties of men.' As well might the maker of phrases say that fire is the builder of the forest, for only in the flame of destruction do we realize the warmth and strength that lie in the heart of oak. Another writer, Hardwick, declares that 'War is essential to the life of a nation; war strengthens a nation morally, mentally and physically.' Such statements as these set all history at defiance. War can only waste and corrupt. 'All war is bad,' says Benjamin Franklin, 'some only worse than others.' 'War has its origin in the evil passions of men,' and even when unavoidable or righteous, its effects are most forlorn. The final effect of each strife for empire has been the degradation or extinction of the nation which led in the struggle.

XXIX. Greece died because the men who made her glory had all passed away and left none of their kin, and therefore none of their kind. ''Tis Greece, but living Greece no more,' for the Greek of to-day, for the most part, never came from the loins of Leonidas or Miltiades. He is the son of the stable-boys and scullions and slaves of the day of her glory, those of whom imperial Greece could make no use in her conquest of Asia. "Most of the old Greek race," says Mr. W. H. Ireland, has been swept away, and the country is now inhabited by persons of Slavonic descent. Indeed, there is strong ground for the statement that there was more of the old heroic blood of Hellas in the Turkish army of Edhem Pasha than in the soldiers of King George, who fled before them three years ago." King George himself is only

an alien placed on the Grecian throne to suit the convenience of the outside powers, which to the ancient Greeks were merely factions of barbarians. In the late war some poet, addressing the spirit of ancient Greece, appealed to her

> "Of all thy thousands grant us three
> To make a new Thermopylæ."

But there were not even three—not even one—'to make another Marathon,' and the Turkish troops swept over the historic country with no other hindrance than the effortless deprecation of Christendom.

XXX. Why did Rome fall? It was not because untrained hordes were stronger than disciplined legions. It was not that she grew proud, luxurious, corrupt, and thereby gained a legacy of physical weakness. We read of her wealth, her extravagance, her indolence and vice, but all this caused only the downfall of the enervated, the vicious and the indolent. The Roman legions did not riot in wealth. The Roman generals were not all entangled in the wiles of Cleopatra.

XXXI. 'The Roman Empire,' says Seeley, 'perished for want of men.' You will find this fact on the pages of every history, though few have pointed out war as the final and necessary cause of the Roman downfall. In his recent noble history of the 'Downfall of the Ancient World' ('Der Untergang der Antiken Welt,' 1897), Prof. Otto Seeck,* of Greifeswald, makes this fact very apparent. The cause of the fall of Rome is found in the 'Extinction of the Best' ('Die Ausrottung der Besten'), and all that remains to the historian is to give the details of this extermination. He says 'In Greece a wealth of spiritual power went down in the suicidal wars.' In Rome "Marius and Cinna slew the aristocrats by hundreds and thousands. Sulla destroyed no less thoroughly the democrats, and whatever of noble blood survived fell as an offering to the proscription of the triumvirate." "The Romans had less of spontaneous power to lose than the Greeks, and so desolation came to them all the sooner. He who was bold enough to rise politically was almost without exception thrown to the ground. *Only cowards remained, and from their brood came forward the new generations.* Cowardice showed itself in lack of originality and slavish following of masters and traditions." Had the Romans been still alive, the Romans of the old republic, neither inside nor outside forces could have worked the fall of Rome. But the true Romans passed away early. Even Cæsar notes the 'dire scarcity of men.' *"δεινήν ὀλιγανθροπίαν."*) Still there were always men in plenty, such as they were. Of this there is abundant testimony. Slaves and camp followers were always in evidence. It was the men of strength and

* I am indebted to Prof. E. A. Ross for the reference to this excellent work.

character, 'the small farmers,' the 'hardy dwellers on the flanks of the Apennines,' who were gone.

"The period of the Antonines was a period of sterility and barrenness. The human harvest was bad." Augustus offered bounties on marriage until 'Celibacy became the most comfortable and most expensive condition of life.' "Marriage," says Metellus, "is a duty which, however painful, every citizen ought manfully to discharge."

"The mainspring of the Roman army," says Hodgkin, "for centuries had been the patient strength and courage, capacity for enduring hardships, instinctive submission to military discipline of the population which lined the ranges of the Apennines."

Berry states that an "effect of the wars was that the ranks of the small farmers were decimated, while the number of slaves who did not serve in the army multiplied." Thus *'Vir gave place to Homo,'* real men to mere human beings.

With the failure of men grew the strength of the mob, and of the emperor, its exponent. "The little finger of Constantine was stronger than the loins of Augustus." At the end "the barbarians settled and peopled the Roman Empire rather than conquered it." "The Roman world would not have yielded to the barbaric were it not decidedly inferior in force." Through the weakness of men, the Emperor assumed divine right. Dr. Zumpt says, "Government having assumed godhead, took at the same time the appurtenances of it. Officials multiplied. Subjects lost their rights. Abject fear paralyzed the people, and those that ruled were intoxicated with insolence and cruelty."

"The Emperor," says Professor Seeley, "possessed in the army an overwhelming force over which citizens had no influence, which was totally deaf to reason or eloquence, which had no patriotism, because it had no country, which had no humanity, because it had no domestic ties." "There runs through Roman literature a brigand's and a barbarian's contempt for honest industry." "The worst government is that which is most worshipped as divine."

So runs the word of the historian. The elements are not hard to find. Extinction of manly blood; extinction of freedom of thought and action; increase of wealth gained by plunder; loss of national existence.

XXXII. So fell Greece and Rome, Carthage and Egypt, the Arabs and the Moors, because, their warriors dying, the nation bred real men no more. The man of the strong arm and the quick eye gave place to the slave, the pariah, the man with the hoe, whose lot changes not with the change of dynasties.

XXXIII. Other nations of Europe may furnish illustrations in greater or less degree. Germany guards her men, and reduces the waste of war to a minimum. She is 'military, but not warlike,' and this dis-

tinction means a great deal from the point of view of this discussion. In modern times the greatest loss of Germany has been not from war, but from emigration. If the men who have left Germany are of higher type than those who remain at home, then the blood of the nation is impoverished. That this is the case the Germans in Germany are usually not willing to admit. On the other hand, those competent to judge the German-American find no type of men in the Old World his mental or physical superior.

The tendency of emigration, whether to cities or to other countries, is to weaken the rural population. An illustration of the results of checking this form of selection is seen in the Bavarian town of Oberammergau. This little village, with a population not exceeding fifteen hundred, has a surprisingly large number of men possessing talent, mental and physical qualities far above the average even in Germany. The cause of this lies in the Passion Play, for which for nearly three centuries Oberammergau has been noted. The best intellects and the noblest talents that arise in the town find full scope for their exercise in this play. Those who are idle, vicious or stupid are excluded from it. Thus, in the long run, the operation of selection is to retain those whom the play can use and to exclude all others. To weigh the force of this selected heredity we have only to compare the quality of Oberammergau with that of other Bavarian towns, as, for example, her sister village of Unterammergau, some two miles lower down, in the same valley.

XXXIV. Switzerland is the land of freedom—the land of peace. But in earlier times some of the thrifty cantons sent forth their men as hireling soldiers to serve for pay under the flag of whomsoever might pay their cost. There was once a proverb in the French Court, 'pas d'argent; pas de Suisses,' no money; no Swiss, for the agents of the free republic drove a close bargain.

In Luzerne stands one of the noblest monuments in all the world, the memorial of the Swiss guard of Louis XVI., killed by the mob at the palace of Versailles. It is carved in the solid rock of a vertical cliff above a great spring in the outskirts of the city. A lion of heroic size, a spear thrust through its body, guarding in its dying paws the Bourbon lilies and the shield of France. And the traveler, Carlyle tells us, should visit Luzerne and her monument, "Not for Thorwaldsen's sake alone, but for the sake of the German Biederkeit and Tapferkeit, the valor which is worth and truth, be it Saxon, be it Swiss."

Beneath the lion are the names of those whose devotion it commemorates. And with the thought of their courage comes the thought of the pity of it, the waste of brave life in a world that has none too much. It may be fancy, but it seems to me that as I go about in Switzerland I can distinguish by the character of the men who remain those

cantons who sent forth mercenary troops from those who kept their own for their own upbuilding. Perhaps for other reasons than this Lucerne is weaker than Graubünden, and Unterwalden less virile than little Appenzell. In any event, the matter is worthy of consideration, for this is absolutely certain: just in proportion to its extent and thoroughness is military selection a cause of decline.

XXXV. Holland has become a nation of old men, rich, comfortable and unprogressive. Her sons have died in the fields of Java, the swamps of Achin, wherever Holland's thrifty spirit has built up nations of slaves. It is said that Batavia alone has a million of Dutch graves. The armies of Holland to-day are recruited in every port. Dutch blood is too precious to be longer spilled in her enterprises.

XXXVI. Spain died of empire centuries ago. She has never crossed our path. It was only her ghost which walked at Manila and Santiago. In 1630, the Augustinian friar La Puente thus wrote of the fate of Spain: "Against the credit for redeemed souls I set the cost of Armadas and the sacrifice of soldiers and friars sent to the Philippines. And this I count the chief loss, for mines give silver, and forests give timber, but only Spain gives Spaniards, and she may give so many that she may be left desolate and constrained to bring up strangers' children instead of her own." "This is Castile," said a Spanish knight; "she makes men and wastes them." "This sublime and terrible phrase," says Lieutenant Carlos Gilman Calkins, from whom I have received both these quotations, "sums up Spanish history."

The warlike nation of to-day is the decadent nation of to-morrow. It has ever been so, and in the nature of things it must ever be.

XXXVII. In his charming studies of 'Feudal and Modern Japan,' Mr. Arthur Knapp returns again and again to the great marvel of Japan's military prowess after more than two hundred years of peace. It is astonishing to him that after more than six generations in which physical courage has not been demanded, these virile virtues should be found unimpaired. We can readily see that this is just what we should expect. In times of peace there is no slaughter of the strong, no sacrifice of the courageous. In the peaceful struggle for existence there is a premium placed on these virtues. The virile and the brave survive. The idle, weak and dissipated go to the wall. If after two hundred years of incessant battle Japan still remained virile and warlike, that would indeed be the marvel. But that marvel no nation has ever seen. It is doubtless true that warlike traditions are most persistent with nations most frequently engaged in war. But the traditions of war and the physical strength to gain victories are very different things. Other things being equal, the nation which has known least of war is the one most likely to develop the 'strong battalions' with whom victory must rest.

XXXVIII. What shall we say of England and her hundred petty wars 'smouldering' in every part of the globe?

Statistics we have none, and no evidence of tangible decline that Englishmen will not indignantly repudiate. Besides, in the struggle for national influences, England has had many advantages which must hide or neutralize the waste of war. In default of facts unquestioned, we may appeal to the poets, letting their testimony as to the reversal of selection stand for what it is worth. Kipling tells us of the cost of the rule of the sea:

> "We have fed our sea for a thousand years,
> And she calls us, still unfed:
> Though there's never a wave of all her waves
> But marks our English dead."
>
> "If blood be the price of admiralty,
> Lord God, we have paid it in full."

Again, referring to dominion on land, he says:

> "Walk wide of the widow of Windsor,
> For half of creation she owns,
> We've bought her the same with the sword and the flame,
> And we've salted it down with our bones.
> Poor beggars, it's blue with our bones."

Finer than this are the lines in the 'Revelry of the Dying,' written by a British officer, Bartholomew Dowling, it is said, who died in the plague in India:

> "Cut off from the land that bore us,
> Betrayed by the land we find:
> When the brightest are gone before us
> And the dullest are left behind.
> So stand to your glasses steady,
> Tho' a moment the color flies,
> Here's a cup to the dead already
> And huzza for the next that dies!"

The stately "Ave Imperatrix' of Oscar Wilde, the last flicker of dying genius in his wretched life, contains lines that ought not to be forgotten:

> "O thou whose wounds are never healed,
> Whose weary race is never run;
> O Cromwell's England, must thou yield
> For every foot of ground a son?
>
> "What matter if our galleys ride
> Pine forest-like on every main;
> Ruin and wreck are at our side,
> Stern warders of the house of pain.

"Where are the brave, the strong, the fleet,
The flower of England's chivalry?
Wild grasses are their winding sheet,
And sobbing waves their threnody.

"Peace, peace, we wrong our noble dead
To vex their solemn slumber so;
But childless and with thorn-crowned head,
Up the steep road must England go!"

We have here the same motive, the same lesson which Byron applies to Rome:

"The Niobe of Nations—there she stands,
Crownless and childless in her voiceless woe,
An empty urn within her withered hands,
Whose sacred dust was scattered long ago!"

XXXIX. It suggests the inevitable end of all empire, of all dominion of man over man by force of arms. More than all who fall in battle or are wasted in the camps, the nation misses the 'fair women and brave men' who should have been the descendants of the strong and the manly. If we may personify the spirit of the nation, it grieves most not over its 'unreturning brave,' but over those who might have been, but never were, and who, so long as history lasts, can never be.

XL. Against this view is urged the statement that the soldier is not the best, but the worst, product of the blood of the English nation. Tommy Atkins comes from the streets, the wharves, the graduate of the London slums, and if the empire is 'blue with his bones,' it is, after all, to the gain of England that her better blood is saved for home consumption, and that, as matters are, the wars of England make no real drain of English blood.

In so far as this is true, of course the present argument fails. If war in England is a means of race improvement, the lesson I would read does not apply to her. If England's best do not fall on the field of battle, then we may not accuse war of their destruction. The fact could be shown by statistics. If the men who have fallen in England's wars, officers and soldiers, rank and file, are not on the whole fairly representative of 'the flower of England's chivalry,' then fame has been singularly given to deception. We have been told that the glories of Blenheim, Trafalgar, Waterloo, Majuba Hill, were won by real Englishmen. And this in fact is the truth. In every nation of Europe the men chosen for the army are above the average of their fellows. The absolute best doubtless they are not; but still less are they the worst. Doubtless, too, physical excellence is more considered than moral or mental strength, and certainly again the more noble the cause, the more worthy the class of men who will risk their lives for it.

Not to confuse the point by modern instances, it is doubtless true

that better men fell on both sides when 'Kentish Sir Byng stood for the King' than when the British arms forced the opium trade on China. No doubt, in our own country, better men fell at Bunker Hill or Cowpens than at Cerro Gordo or Chapultepec. The lofty cause demands the lofty sacrifice.

It is the shame of England that most of her many wars in our day have cost her very little. They have been scrambles of the mob or with the mob, not triumphs of democracy.

There was once a time when the struggles of armies resulted in a survival of the fittest, when the race was indeed to the swift and the battle to the strong. The invention of 'villainous gunpowder' has changed all this. Except the kind of warfare called guerrilla, the quality of the individual has ceased to be much of a factor. The clown can shoot down the hero and 'doesn't have to look the hero in the face as he does so.' The shell destroys the clown and hero alike, and the machine gun mows down whole ranks impartially. There is little play for selection in modern war save what is shown in the process of enlistment.

XLI. America has grown strong with the strength of peace, the spirit of democracy. Her wars have been few. Were it not for the mob spirit, they would have been still fewer, but in most of them she could not choose but fight. Volunteer soldiers have swelled her armies, men who went forth of their own free will, knowing whither they were going, believing their acts to be right, and taking patiently whatever the fates may hold in store.

The feeling for the righteousness of the cause, "with the flavor of religion in it," says Charles Ferguson, "has made the volunteer the mighty soldier he has always been since the days of Naseby and Marston Moor." Only with volunteer soldiers can democracy go into war. When America fights with professional troops, she will be no longer America. We shall then be, with the rest of the militant world, under mob rule. "It is the mission of democracy," says Ferguson again, "to put down the rule of the mob. In monarchies and aristocracies it is the mob that rules. It is puerile to suppose that kingdoms are made by kings. The king could do nothing if the mob did not throw up its cap when the king rides by. The king is consented to by the mob because of that which in him is mob-like. The mob loves glory and prizes. So does the king. If he loved beauty and justice, the mob would shout for him while the fine words were sounding in the air; but he could never celebrate a jubilee or establish a dynasty. When the crowd gets ready to demand justice and beauty, it becomes a democracy, and has done with kings."

It was at Lexington that the embattled farmers 'fired the shot that was heard around the world.' To them life was of less value than a

principle, the principle written by Cromwell on the statute book of Parliament: 'All just powers under God are derived from the consent of the people.' Since this war many patriotic societies have arisen, finding their inspiration in personal descent from those who fought for American independence. The assumption, well justified by facts, is that these were a superior type of men, and that to have had such names in our personal ancestry is of itself a cause for thinking more highly of ourselves. In our little private round of peaceful duties, we feel that we might have wrought the deeds of Putnam and Allen, of Marion and Greene, of our revolutionary ancestors, whoever they may have been. But if those who survived were nobler than the mass, so also were those who fell. If we go over the record of brave men and wise women whose fathers fought at Lexington, we must think also of the men and women who shall never be, whose right to exist was cut short at this same battle. It is a costly thing to kill off men, for in men alone can national greatness consist.

XLII. But sometimes there is no other alternative. It happened once that for 'every drop of blood drawn by the lash another must be drawn by the sword.' It cost us a million of lives to get rid of slavery. And this million, North and South, was the 'best that the nation could bring.' North and South, the nation was impoverished by the loss. The gaps they left are filled to all appearance. There are relatively few of us left to-day in whose hearts the scars of forty years ago are still unhealing. But a new generation has grown up of men and women born since the war. They have taken the nation's problems into their hands, but theirs are hands not so strong or so clean as though the men that are stood shoulder to shoulder with the men that might have been. The men that died in 'the weary time' had better stuff in them than the father of the average man of to-day.

Read again Brownell's rhymed roll of honor, and we shall see its deeper meaning:

> Allen, who died for others,
>   Bryan of gentle fame,
> And the brave New England brothers
>   Who have left us Lowell's name;
> Bayard, who knew not fear,
>   True as the knight of yore,
> And Putnam and Paul Revere,
>   Worthy the names they bore.
> Wainwright, steadfast and true,
>   Rodgers of brave sea-blood,
> And Craven, with ship and crew,
>   Sunk in the salt-sea flood.
> Terrill, dead where he fought,
>   Wallace, that would not yield;
> Sumner, who vainly bought
>   A grave on the foughten field,

But died ere the end he saw,
  With years and battles outworn;
There was Harmon of Kennesaw,
And Ulric Dahlgren, and Shaw
  That slept with his Hope Forlorn.
Lytle, soldier and bard,
  And the Ellets, sire and son,
Ransom, all grandly scarred,
And Redfield, no more on guard;
  But Alatoona is won!

So runs the record, page after page:

"All such, and many another,
  Ah, list, how long to name!"

And these were the names of the officers only. Not less worthy were the men in the ranks. It is the paradox of democracy that its greatness is chiefly in the ranks. "Are all the common men so grand, and all the titled ones so mean?"

XLIII. North or South, it was the same. 'Send forth the best ye breed' was the call on both sides alike, and to this call both sides alike responded. As it will take 'centuries of peace and prosperity to make good the tall statures mowed down in the Napoleonic wars,' so like centuries of wisdom and virtue are needed to restore to our nation its lost inheritance of patriotism. Not the capacity for patriotic talk, for of that there has been no abatement, but of that faith and truth which 'on war's red touchstone rang true metal.' With all this we can never know how great is our real misfortune, nor see how much the men that are fall short of the men that ought to have been.

It will be said that all this is exaggeration, that war is but one influence among many, and that each and all of these forms of destructive selection may find its antidote. This is very true. The antidote is found in the spirit of democracy, and the spirit of democracy is the spirit of peace. Doubtless these pages constitute an exaggeration. They were written for that purpose. I would show the 'ugly, old and wrinkled truth stripped clean of all the vesture that beguiles.' To see anything clearly and separately is to exaggerate it. The naked truth is always a caricature unless clothed in conventions, fragments taken from lesser truths. The moral law is an exaggeration, 'The soul that sinneth it shall die.' Doubtless one war will not ruin a nation; doubtless it will not destroy its virility or impair its blood. Doubtless a dozen wars may do all this. The difference is one of degree alone; I wish only to point out the tendency. That the death of the strong is a true cause of the decline of nations is a fact beyond cavil or question. The 'man who is left' holds always the future in his grasp. One of the great books of our new century will be some day written on the selection of men, the screening of human life through the actions of man and the operation

of the institutions men have built up. It will be a survey of the stream of social history, its whirls and eddies, rapids and still waters, and the effect of each and all of its condtions on the heredity of men. The survival of the fit and the unfit in all degrees and conditions will be its subject matter. This book will be written, not roughly and hastily, like the present fragmentary essay, still less will it be a brilliant effort of some analytical imagination. It will set down soberly and statistically the array of facts which as yet no one possesses, and the new Darwin whose work it shall be must, like his predecessor, spend twenty-five years in the gathering of 'all facts that can possibly bear on the question.' When such a book is written, we shall know for the first time the real significance of war.

XLIV. If any war is good, civil war must be best. The virtues of victory and the lessons of defeat would be kept within the nation. This would protect the nation from the temptation to fight for gold or trade. Civil war under proper limitations could remedy this. A time limit could be adopted, as in football, and every device known to the arena could be used to get the good of war and to escape its evils.

For example, of all our States New York and Illinois have doubtless suffered most from the evils of peace, if peace has evils which disappear with war. They could be pitted against each other, while the other States looked on. The 'dark and bloody ground' of Kentucky could be made the arena. This would not interfere with trade in Chicago, nor soil the streets in Baltimore. The armies could be filled up from the ranks of the unemployed, while the pasteboard heroes of the national guard could act as officers. All could be done in decency and order, with no recriminations and no oppression of an alien foe. We should have all that is good in war, its pomp and circumstance, the 'grim resolution of the London clubs,' without war's long train of murderous evils. Who could deny this? And yet who could defend it?

If war is good, we should have it regardless of its cost, regardless of its horrors, its sorrows, its anguish, havoc and waste.

But it is bad, only to be justified as the last resort of 'mangled, murdered liberty,' a terrible agency to be evoked only when all other arts of self-defense shall fail. The remedy for most ills of men is not to be sought in 'whirlwinds of rebellion that shake the world,' but in peace and justice, equality among men, and the cultivation of those virtues we call Christian, because they have been virtues ever since man and society began, and will be virtues still when the era of strife is past, and the 'redcoat bully in his boots' no longer 'hides the march of man from us.'

It is the voice of political wisdom which falls from the bells of Christmas-tide: "Peace on earth; good will towards men!"

PROGRESS AND TENDENCY OF MECHANICAL ENGINEERING IN THE NINETEENTH CENTURY.—II.

BY PROFESSOR ROBERT H. THURSTON,
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IN 1800, Galvani and Volta had sewed the seed, and since has sprung up the whole science and art of electrical physics. Ten years ago we had about 700 miles of electric railway; to-day about 15,000 miles are in operation in the United States alone; a thousand millions of dollars are invested in the stock, and an army of two hundred thousand men is employed by them, mainly in the great cities, but with steady growth towards all sections and into all aggregations of population. Two thousand millions of dollars are reported to be now invested in apparatus of electrical distributions of energy, converted ultimately into light and power. About two-thirds of a *billion* of dollars are invested in the property of the electric light companies. We have between one and two million miles of telephone wire, and can talk from Boston to Chicago; from Chicago to San Francisco will soon be found an easy conversational distance. The Bell Company alone owns a million miles of wire, a million and a half instruments, and receives six millions of dollars a year from its business. The world, outside the United States, utilizes not quite as much capital in this most wonderful of the inventions of the century as does our own country, having about a half-million exchanges to our six hundred thousand and over on the Bell system alone.

Of steam power, about twenty millions of the engineers' 'horse-power,' the equivalent of perhaps seventy-five, or even possibly more nearly a hundred, millions of horse-power developed by animal forces, move the fleets of the world, merchant and naval, and drive our ships across every sea. It even has been found practicable to apply steam-power to the sewing machine, and of the million or more manufactured in the United States and the fifty per cent. added to the total by other nations, a very considerable fraction are operated by steam-power, and of the hundred thousand people engaged in its manufacture and the millions engaged in its use, a corresponding proportion are aided by this mighty engine of civilization. Steam supplies the power for driving the machinery which produces a quarter of a million mowers and reapers in the United States—an unknown industry a century ago—and thus, with the help of the steam-plow and other machinery of agriculture, all inventions of the century, secures for the nation a foreign market for

two hundred millions of dollars' worth of grain and flour, a surplus left us after feeding our own population as the people of no other country are, or ever were, fed. Farms of tens of thousands of acres in area can now be thus cheaply cultivated.

Electrical engineering is to-day one of the most impressive of all modern developments in mechanical engineering, and the whole world is coming to be served by the installation of the machinery of our light and power distribution 'plants.' While it is true, as often remarked, that electrical engineering is not only a department of mechanical engineering, but one which involves, in large proportion, design, construction and operation in the more familiar departments of mechanical engineering as fundamental bases, it is none the less true that electrical engineering is most closely approximate to pure science and most distinctive in its own character among all specialties taken up by the engineer as individual vocations. The machinery of the business involves all the principles of design and construction taught the mechanical engineer, and the scientific side, once almost purely such, now attaches itself to the mechanical as a lesser to a greater. The whole of this enormous accession to the world's industries has come in within the last half-century, practically, and the telegraph, the telephone, the electric light and the electric railway have succeeded one another since that date. The last is the outcome of the last quarter-century.

The energy which carries the telegram along the wires to-day comes from the steam-engine, which is now a principal and most absolutely essential element; telephones, like telegraph instruments, are the output of most extensive and important manufacturing establishments; electric light and power distributions are all systems of distribution of the power of the steam-engine. To-day there are probably $3,000,000,000 invested, in our country alone, in telegraphs, telephones and electric distributions, of which the larger part by far is invested in the latter. In fact, Mr. T. C. Martin reckons a still larger total, and computes these figures: telegraph, $250,000,000; telephones, $300,000,000; electric lighting, $1,200,000,000; electric railways, $1,800,000,000; other uses of electric power, $250,000,000; manufacturing, $150,000,000; storage batteries, etc., $25,000,000; total, $3,975,000,000, about four thousand millions, nearly four *billions*, of dollars.

More seductive even than the problems of the electrical engineer, more deceitfully promising than any one of the great problems of the age, seemingly more completely solved in its subsidiary elements and almost on the very verge of solution, completely and perfectly, is the task assigned the inventor from the earliest days of the world, from the day when the first man saw the first bird rise from under his feet and wing its way toward the heavens, safe, free and joyous: the problem of aërodromics, of aviation and aëronautics. Inventors attacked this prob-

lem in prehistoric times, and have never ceased their endeavors; for there is no invention, and never has been imagined an invention more attractive to the mind of man; nor is there any invention the perfection of which would have more interest for mankind or illustrate more splendidly the triumph of the mind of man over the conditions which hem him in. Yet the century which has seen such marvelous, almost catastrophic, evolutions in all other fields has seen its end without final success here.

Yet some important advances have been made. The dirigible balloon has become capable of contending with moderate winds, and of traversing still air in any direction at moderate speed and for small distances; the balloon itself and its motors are taking definite form and standard proportions, especially in the hands of the military staff of the armies of European nations. Our own army officers have not, so far as known, entered upon this task, though having at hand the most royal inventors of the world. Count Zeppelin probably illustrates the furthest advance in this department.

In aërodromics, Professor Langley has completely developed the fundamental principles of self-sustaining flight, and has revealed the fact that there are far fewer and far less formidable obstacles to be overcome in this direction than had been previously supposed. His researches are the classics of this division of applied science, and his experimental investigations of the laws of this science will permanently stand as the first important steps in the development of the rational basis of all future work, and as the foundations of aërodromic science; while his extraordinary work in the practical evolution of the aërodrome —the more wonderful as the work of a scientific man whose vocations, and until recently whose avocations, have been in quite other departments than those of mechanical construction—will always remain famous as the first deliberate and successful attempt to carry into practice principles thus revealed. In the nineteenth century, we may at least claim, these first advances on firm grounds have been effected, and we need not be at all surprised if, in the earlier years of the new century, complete success, so far as the mechanical engineering of the case is concerned, shall be attained. There is some reason to doubt whether commercial success will follow—not that it is in itself inherently impossible, but that it is a question whether, in the presence of the competition of the more advantageous methods of transportation on solid land and with the buoyant and hardly less effective support of the ocean wave, conveyance of passengers and of merchandise can not always be generally effected vastly more safely and cheaply. Yet that there will be found a place and purpose for aviation, in time of war if not in time of peace, and even probably for profitable employment, we may not doubt.

Steam is apparently coming, as the various other motor-fluids are, into use on the highway, and, after an interregnum of a half-century or more, due to the stupidity of legislators mainly, the automobile, in innumerable forms and on innumerable 'systems,' is once again displacing the horse in city streets and, in less degree, perhaps, on country roads, and is promising ere long to do a large part of our transportation of merchandise over short routes and off the line of the railway. Thousands are now in use in this country and abroad, and tens—hundreds, nominally—of millions of dollars are invested in their manufacture.

In infinite variety the progress of invention thus reveals itself. Individuals, nations, even continents and worlds have courses like that of the rocket: rising with rapid acceleration, upward and onward, to a culmination, when a sudden development of energy from latent form occurs, and a brilliant illumination for the moment surprises and enlightens us; then the limit is attained, the path curves over and downward, and after a brief period, downward acceleration begins and its career presently comes to an end. This is not the history of invention, which is never self-limited; a step made is never retraced. The progress of the day is not only recorded in written and printed history and permanently preserved, but is given a still more permanent record in the life and habits and traditions of the people, and each invention and each new advance is the basis of a later and still higher progress. The only limit to be expected to this advancement of civilization, through invention and the mechanic arts, is that set by some catastrophe which shall ultimately involve the life of the race, having its source in natural evolutions of a physical character, and bringing to an end all the activities of mankind in some probably far-distant generation.

The extent to which the specialization consequent upon the changes in the mechanism and methods of manufactures have progressed during the century just past, may be realized more fully when it is understood that, for example, in the making of a watch, there may be fifteen or sixteen hundred operations conducted with the help of five or six hundred machines by as many operatives, each of whom necessarily acquires wonderful expertness in tasks thus repeated constantly, hour by hour, day by day, throughout the working day and the calendar year. These movements become intuitive and automatic; their accuracy and rapidity become almost incredible, and the human machine, through its internal automatism, thus relieves the mind and gives it freedom from stress and fatigue in a manner unknown to the worker of earlier days. The labor-assisting machinery also thus enables the operative to produce, without serious toil and fatigue, from ten to a hundred times as much of his special product as could his unaided predecessor in the vocation with, however, more concentrated attention giving far less skill and accuracy. The inventor and the mechanic thus illustrate the immense

difference in value and efficiency to be observed between work of brain and work of muscle alone.

Meantime the worker receives larger wages; each dollar will buy more of the necessaries of life, vastly more of its comforts. Clothing is better, cheaper and more plentiful; food is better, of greater variety and is easier obtained; wages have gone up and prices have gone down; the average citizen finds it easier to secure employment at remunerative wages; he secures a larger and a larger proportion of the earnings of capital and labor, and he obtains more opportunities for incidental profit and for paying investments of his more easily acquired savings. The savings banks of the country are now finding difficulty in caring for his accumulations, while the larger capitalist is finding no less difficulty in securing a fair return on invested capital in large amounts.

Twenty years ago, when preparing the second annual address of the then President of the American Society of Mechanical Engineers, I wrote:*

"I have sometimes said that the world was waiting for the appearance of three great inventors, yet unknown, for whom it has in store honors and emoluments far exceeding all ever yet accorded to any one of their predecessors.

"The first is the man who is to show how, by the consumption of coal, we may directly produce electricity, and thus, perhaps, evade that now inevitable and enormous loss that comes of the utilization of energy in all heat-engines driven by substances of variable volume. Our electrical engineers have this great step still to take, and are apparently not likely soon to gain the prize that may yet reward some genius yet to be born.

"The second of these greatest inventors is he who will teach us the source of the beautiful soft-beaming light of the firefly and the glow-worm, and will show us how to produce this singular illuminant, and to apply it with success practically and commercially. This wonderful light, free from heat and from consequent loss of energy, is nature's substitute for the crude and extravagantly wasteful lights of which we have, through so many years, been foolishly boasting. The dynamo-electrical engineer has nearly solved this problem. Let us hope that it may be soon fully solved, and by one of those among our own colleagues who are now so earnestly working in this field, and that we may all live to see him steal the glow-worm's light, and to see the approaching days of Vril predicted so long ago by Lord Lytton.

"The third great genius is the man who is to fulfil Darwin's prophecy (1759), closing the stanza:

"Soon shall thy arm, unconquered steam, afar
Drag the slow barge or drive the rapid car,
Or, on wide-waving wings expanded bear
The flying chariot through the fields of air."

Of these three inventors none has yet appeared, and their coming may prove to be the great events of the twentieth century. The task set for the first has been often attacked by later men of science, and especially the chemists; but, while some real progress has been made, the purpose of this inventor is not accomplished and seems little, if any,

*Trans. Am. Soc. M. E.—1881.

nearer accomplishment than at the end of the last quarter-century. But the time will yet come, we at least may reasonably hope, if not predict, when a way will be found thus to increase the availability of the stored energy of our fuel deposits, until they shall furnish ten times the power and energy now obtainable from each ton of fuel; thus correspondingly lengthening the period of human life and work in the temperate regions of the earth. Were this to-day possible, the endurance of the Pennsylvania coal-beds as sources of power would be lengthened from the present anticipated century to a millennium, and the thirtieth century, instead of only the twentieth, would profit by them. Great Britain might hope to continue a manufacturing nation for five centuries to come, and the world might gain ten times as much permanent wealth, by its use of the latent energy of fuel, as now seems possible.

The mechanical engineer, the electrician and the chemist have here an incentive to a most magnificent task and a noble rivalry.

The second of our great triumvirate of inventors or discoverers is more certainly coming. His advent is indicated by the electrical engineer and the physicist in their use of electrical energy of enormously high tension; while the biological chemist is now a close second in the race, through his researches in the field of low-temperature combustion and amongst the animal forms producing light and electricity without heat—the animal machines in which the processes of nature are seen already accomplishing the task. This being done, the engineer will be able to reduce the cost of lighting, as measured in power, to one-twentieth its present amount, and as measured in fuel, if he can combine these two improvements effectively, to one-two-hundredth its amount to-day, proportionally reducing the intimidating waste now going on in our deposits of irreplaceable natural stores of power.

The third inventor is also here with a crude beginning of his task, and while, at the commencement of the nineteenth century, he was a subject of unsparing ridicule, and even sometimes by able men within the last decade, he would be a bold man who should to-day dare to assert the improbability of the coming century seeing the problem solved, so far as its engineering is concerned. The commercial problem must be left to take care of itself—as it always has done hitherto.

All these are evidently problems affecting vitally all progress in the future of energy-production in the field of mechanical engineering. When complete conversion of energy is effected by any mechanism employing our natural sources of energy, the task of the builder of the air-ship is rendered less difficult, the cost of light-production is made easier and the utilization of the latent energy of fuel through the heat-engine is made comparatively insignificant in cost.

This much is revealed to us through 'The Great Discovery of the Age,' as some one has rightly called it: the discovery and experimentally

confirmed 'Law of Substance,' as Haeckel denominates it, the principle in nature which I enunciated a quarter of a century ago thus:

"All that exists, whether matter or force, or their product, energy, and in whatever form, is indestructible except by the infinite power which has created it."*

This principle, probably as old as Aristotle, or older, enunciated by Cicero when he declared, "One eternal and immutable law embraces all things and all times;" experimentally proved, at least qualitatively, by Rumford in the latter part of the eighteenth century, confirmed by Davy, proved and quantitatively illustrated by Mayer, by Joule and by Rowland and numerous contemporary investigators, the Law of Substance of Haeckel, is itself a nineteenth century product and the basis of our whole system of energy production, transmutation and transmission, the foundation of the whole superstructure in mechanical engineering and of its wealth-production, and of human progress and higher human life.

Education in applied science and in the principles directly underlying the work of the engineer, in common schools, secondary schools and professional schools and colleges, an education which has seen as much improvement as have the arts and sciences themselves, has had much to do with the later progress of mechanical engineering, especially in the United States. Systematic instruction in the departments of mechanical engineering, such as is now obtainable by almost any young man determined to secure it, not only has much to do with our progress at the moment, but it is this phase of education, in our state colleges particularly, which is settling the tendency of the flow of the rising tide for the immediate future, and probably for all coming time. Although it has been a force of recognized importance and influence for less than a single generation, and has had a distinct and special position among 'the educations' for a very brief period, it has already done much to correct the defects of the industrial system of our country—still more that of France and that of Germany, hardly less that of Great Britain—and also to systematize our industries. The discoveries of science and the inventions of our mechanics furnish material to be utilized by the alumni of our technical and professional schools and colleges as they can be by no other class in the community; the scientific method of the schools and the scientific knowledge of their graduates, and the hands and brains of the new leaders of the industrial army give perfected organization and improved administration to every branch of the great economical, machine-like, modern industrial sys-

* Proceedings of Am. Assoc. for Advancement of Science, 1878; Vice-Presidential Address: 'The Scientific Method of Advancement of Science.'—R. H. T.

Also 'Manual of the Steam-Engine,' Vol. I, Chap. IV, § 75, p. 299, THE POPULAR SCIENCE MONTHLY, March, 1901.

tem. Even where these well-trained officers are not in command, their influence is felt, and every member of the organization works in accordance with their more efficient systems. The whole nation is rapidly learning how to make the most and best of its powers, as well as how to profit by growing opportunities and acquisitions.

Thomas Huxley, admittedly an authority on the subject of scientific training, said, in his Mason College address:

> "Neither the discipline nor the subject-matter of classical education is of such direct value to the student of physical science as to justify the expenditure of valuable time on either." . . . "For the purpose of attaining real culture, an exclusively scientific education is at least as effectual as an exclusively literary education."

Huxley was a member of nearly all the royal commissions on education of his time, and had large opportunities for observation and investigation in this field. His views were founded on extensive and rare experience and sound knowledge; none could speak with greater authority. He says in one of his addresses on this subject:

> "The great mass of mankind have neither the liking nor the aptitude for either literary or scientific or artistic pursuits; nor, indeed, for excellence of any sort. Their ambition is to go through life with moderate exertion and a fair share of ease, doing common things in a common way. And a great blessing and a comfort it is that the majority of men are of this mind; for the majority of things to be done are common things, and are quite well enough done when commonly done. The great end of life is not knowledge, but action. What men need is as much knowledge as they can assimilate and organize into a basis for action; give them more and it may become injurious. One knows people who are heavy and stupid from undigested learning, as others are from over-fulness of meat and drink. But a small percentage of the population is born with that most excellent quality, the desire for excellence, or with special aptitude of some sort or other. . . . Now, the most important object of all educational schemes is to catch those exceptional people and turn them to account for the good of society. No man can say where they will crop up; like their opposites, the fools and the knaves, they appear sometimes in the palace, sometimes in the hovel; but the great thing aimed at, I was almost going to say the important end of all social arrangements, is to keep these glorious sports of Nature from being corrupted by luxury or starved by poverty, and to put them into the positions in which they can do the work for which they are specially fitted. . . . I weigh my words well when I say that if the nation could purchase a potential Watt or Davy or Faraday at the cost of a hundred thousand pounds down, he would be dirt cheap at the money." *

But our modern educations are producing many Watts and Davys and Faradays, and as progress continues and research becomes more and more the privilege of these 'glorious sports of Nature,' and as more and more men of genius become revealed by systematic, scientific education, the outcome must inevitably be a vastly more complete exploration of

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* Mitchell's sketch of The Life and Work of Huxley; Leaders in Science Series; Putnams; 1900; Chap. XI.

the hidden mysteries of natural phenomena and continually more and more rapid development of these as yet unexplored mines. Our Watts and Davys and Faradays are already gradually discovering the secrets of Nature's production of light without heat, of heat without wastes, of electricity within minimum weight and space, making all elements subservient with at least similar, if not equal, effectiveness with that measured by them in the animal machine—the animal machine, still concealing from them many a secret, must soon reveal all, and permit many later Watts and Davys and Faradays to make our stores of natural energies of multifold value and efficiency in the performance of the tasks of the future.

The outcome of the century, so far as our methods of education are concerned, has been the recognition and the introduction of those ideals of intellectual, technical and practical training which were the ideals of Milton and of many another great mind in earlier days, but which had never before been adopted by educators and statesmen. We have at last, however, come to see that

"The type of education and training most effective in rendering the individual most helpful to his fellows is that which gives ability to be helpful. Given the power of effectively aiding others, the sympathies will be found always present; given the means of utilizing generous impulses, they will be found always fruitfully active.

"Teach habits of physical and mental activity, and a healthy body and mind will be prolific of wholesome and noble thought; cultivate skill in fruitful industries, and the inclination to employ that skill in helpful ways will not be lacking: feed the soul with the harvests of thought of all ages, with the gleanings of the wisdom of the centuries—in whatever language, however given verbal expression—and all sympathies, latent or active, will find their destined place and work. Breed 'the soul of the sage in the body of the athlete,' and give the perfected soul, within its perfected body, ability to do for itself and others what life may demand of it, and trust that what may be done most effectively for the world will be done best by this perfected humanity, through the exercise of broadest sympathies and most efficient powers of aiding fellow men.

"It is thus that the Miltonian training, reinforced by Miltonian learning, perfected by Miltonian culture, doing most for the humblest, much for the highest, whether ranked by place or by mind, giving health to the body, skill to eye and hand, stimulus to the intellect, and greatness to the soul, will, always and everywhere, most effectively broaden the sympathies and render the individual most helpful to his fellow men." *

With such education of the people, a nation is assured of permanence and progress. Demagoguism may still poison its legislatures; hysteria may continue to affect its press and here and there a community; amateurism is likely to reduce for a time the efficiency of its public services; but its youth, growing up with a true Miltonian training, with not only learning, but wisdom, not only culture, but directly practical training,

* Miltonian Teaching: an address delivered at Pratt Institute, Brooklyn, December 11, 1894.—R. H. T.

will steadily improve governmental, industrial and educational methods and raise the nation to higher than Platonian levels.

Thus, the Progress and Tendency of Mechanical Engineering have been like that of human life, in many ways. As the outcome of an evolution extending back into an infinite, or at least indefinite, past, its birth, the first sensible evidence of existence, occurred a century ago. Its growth involved the development of many and different phenomena, the perfection of all, in the adult, depending ultimately upon the perfection of each in the process of development. This mighty giant of modern civilization was conceived in liberty, nourished by law. Invention and protecting legislation, assuring to every man the fruit of his brain as of his hand, gave the child health and early and sturdy development. The introduction of new and great inventions in the early part of the century; the formulation in legal terms of our Constitution, of the patent-law and of a system of universal common-school education; the systematization of manufactures, and their care and support, until the advantages of foreign competitors were neutralized; the substitution in all departments of production of automatic and of labor-assisting machinery; the reduction of all productive vocations to scientific departments of mechanical engineering; legal provision for the coöperation of individuals, the invention of the corporation, the later coöperation of corporations in reduction of non-productive labor and the resultant decrease of costs and prices; the introduction of science and of practically applied science into the curriculum of the schools and colleges; the provision of technical and professional schools for the constructive arts and professions; the gravitation of the management of the productive, and of all industrial, operations into the hands of scientifically and practically expert men, who supplement the learning of schools by the perhaps higher learning of the arts and of the professions—all these have illustrated the progress and tendency of mechanical engineering during the nineteenth century and the plainly distinguishable tendency of the time points the way in which the twentieth century is to further illustrate this progress and tendency, in even more marked degree.

In the future, as in the recent past, the progress of invention and of the mechanic arts will undoubtedly be still onward and upward, with a still accelerated motion; the discoveries of the century may be expected to be more important and more imposing than ever before; the work of the world will be performed with a more complete system, and industrial operations of every sort will be carried on on a still larger scale. The now familiar motors, developing the energies of Nature and supplying the power needed to do the work of the world, will be, undoubtedly, still further improved and developed, and it may be hoped, if not fairly expected, that a new method of utilizing the latent powers

of Nature may be discovered, and the requisite mechanism invented, by which to evade the inevitable loss of the larger part of that energy when developed by our present thermo-dynamic machines, and thus to secure the greater part as actually utilized, as useful power. The perfection of our machinery by improvement of details by our later inventors and the production of new and still more wonderfully productive machines, automatic and labor-multiplying, will as certainly continue, and in some fields is likely to astonish us, callous as we have become to these marvels, quite as much as were our parents surprised by the inventions of the last generations. Coöperation of labor, of capital, of manufacturing organizations, will necessarily go on, and the 'captains of industry' will have continually larger and larger armies and greater and greater tasks, and we shall have generals, as well as colonels and captains and subordinates, in the vast armies of united workers of the coming century.

We shall secure a liberal supply of this world's good things by a reasonable day's labor on the part of the humblest, wealth in abundance in repayment for superior talent, industry and forethought, and comfort and a healthful and wholesome life, a happy life, will be assured to all who choose to make the most and best of opportunity, including the wealthy, whose opportunities will be readily found in the promotion of higher learning, of nobler charities and of more generous care of the physically and intellectually lame and halt and blind. We shall increase the speed of our fast trains, cross the Atlantic in less time, transport the food and clothing and wealth of the world more cheaply, and very possibly add to the fields of invention and the practically available means of transportation the long-looked-for department of aëronautics and aërodromics. We have conquered the land and the water; who shall say that Man is less equal than albatross or sparrow to the task of subduing the elements of the atmospheric world?

With further evolution in these departments, and consequent improvement in the condition of the people, all intellectual and moral conditions may be hopefully expected to improve, and the people will grow in character as they acquire knowledge, gaining intelligence as they secure ease of life, and will rise to a higher plane of rectitude and happiness as they are relieved of the grinding pressure of the poverty of earlier times.

Progress has come to be enormously rapid, and the advance of a generation is much greater than was formerly that of centuries. We may reasonably hope to see something of this multiplied progress of the coming generation; our children will see the still larger multiplication of gain of the twentieth century, and help carry the world a long way toward that ideal which has been but feebly described in Plato's Republic and More's Utopia, and has been the aspiration of all good men.

THE PERIODIC LAW.

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BEFORE the time of Lavoisier ideas concerning the nature of matter were mere speculations. Following the introduction, at the close of the eighteenth century, of the conception of the indestructibility of matter, and more especially with the introduction of the atomic theory a decade or so later, the idea of some sixty or seventy absolutely different kinds of matter received general acceptance. The unity of these different elements was, indeed, held by some, but as a pure speculation, while the evidence was all against it. It remained for the Periodic Law to show that there is a connection between these different elements. It is true, we are as far as ever from any knowledge of what that connection is, or from any knowledge of the nature of that primal substance out of which all matter is shaped, unless, indeed, the recent work of J. J. Thomson and others on the electric condition of gases is pointing us thitherward.

The early attempts to classify substances from a chemical, or rather alchemistical standpoint, were wholly superficial. Pliny, for example, describes two forms of lead, *plumbum nigrum* and *plumbum candidum.* The former term was used for lead proper, the latter for tin, though these two metals have little resemblance, except in their low melting points. Sulfuric acid was classed with the oils, as oil of vitriol, and the name has popularly and technically remained to the present, although the only resemblance of sulfuric acid to an oil is in its appearance. The chlorids of antimony and of tin were known respectively as butter of antimony and butter of tin, from the fact that they are semisolid substances, of much the same consistency as butter from milk. Even to-day we speak familiarly of milk of lime and milk of sulfur, though but for the fact that they are whitish liquids, they have nothing in common with the product from the cow. Perhaps to us one of the most remarkable instances of classification was the association of the black oxid of manganese with the white oxid of magnesium, commonly known as calcined magnesia. The only property common to these two, *magnesia nigra* and *magnesia alba,* as they were early called, is that both are fine powders. In the seventeenth and eighteenth centuries the discovery of the different gases began, and to the workers of that day all were but different kinds of air. Thus we find 'inflammable air' as the name for hydrogen, 'fixed air' for carbonic acid gas, and 'dephlogisticated marine acid air' for chlorin. That no better principle of

classification of substances from a chemical standpoint than that of superficial outward appearance was demanded is not strange, when we recollect that at this period and, indeed, down to the close of the eighteenth century, the transmutation of metals was a popular belief of the common people and was not disproved by the chemist. There was no underlying, unchangeable principle at the basis of the different substances with which the chemist had to deal.

Lavoisier—government medallist at twenty-one, adjunct member of the French Academy at twenty-five, chemist, geologist, mineralogist and mathematician, man of business and amasser of wealth, financier, reformer, fermier général, imprisoned by Robespierre on the trumped-up charge of having adulterated tobacco with water, guillotined in 1794, when only just past fifty years old—this is the man whom the French, with much justice, call the 'Father of Chemistry,' the man who made chemistry possible as a science by furnishing it with a foundation, the doctrine of the indestructibility of matter. This he accomplished by the use of the balance. A familiar experiment had often been used to support the old idea of transmutation. When water has been boiled for a long time in a glass vessel, on evaporating the water an earthy residue is obtained, and this, said the chemists of that day, is conclusive evidence that water can be transmuted into earth by boiling; and, if water into earth, why not other substances; and why not, if we only knew the method, even the base metals into gold? When less than thirty years old, Lavoisier repeated this experiment, but he took the precaution of weighing his glass vessel with its contained water. After a hundred days' boiling, he found that there was no change in weight. On then evaporating the water he found, indeed, an earthy residue, but the glass vessel had lost an amount exactly equal in weight to that recovered from the water. In other words, the water, so far from being changed into earth, had merely dissolved out a small portion of the glass container. This and many other similar experiments the keen-witted Frenchman used to prove the indestructibility of matter, and on this fundamental doctrine the superstructure of scientific chemistry began to rise.

With this doctrine established, it became possible to consider the nature of matter from a new standpoint, and to define with some accuracy a chemical element. Back in the days of Greek philosophers, elements were very variously conceived of. To Pherekides earth was the primal element; to Anaximenes, air; to Herakleitos, fire; while Thales found in water the first principle of all things, and the followers of the Milesian philosopher were not a few for more than two millenniums. Empedokles accepted all four of these elements, and to them Aristotle added a fifth, ether, the quintessence, subtler and more divine than the other four. With these the alchemists placed a number of

substances, approaching somewhat our present idea of elements, but even down to Lavoisier's time the old Greek conceptions were not abandoned. Lavoisier's definition of an element deserves to be quoted, since more than a century of chemical progress has failed to improve or in any essential way change it. "An element," says he, in his 'Traité de Chimie,' "is a substance from which no simpler body has as yet been obtained; a body in which no change causes a diminution of weight. Every substance is to be regarded as an element until it is proved to be otherwise." With his conception of an element, Lavoisier introduced a new and scientific nomenclature into chemistry, which is to a very considerable extent in use to-day. The views of Lavoisier did not gain immediate recognition, but a decade after his untimely death the new ideas had been very generally adopted.

With the opening of a new century came the rehabilitation of a theory which had originated back in the misty days of early Greek philosophy, but which was now to be given a new value, because no longer a vague guess, but founded upon experimental evidence. This was the theory of the atomic constitution of matter. According to the Greek conception, if matter were divided into smaller and ever smaller portions, at last a point would be reached where the particles are indivisible, and such particles are the atoms. Dalton seems first to have hazarded the idea of atoms, almost as a speculation, to account (wrongly) for the various different degrees of solubility of different gases in water, and at this early stage he published a table of familiar substances, with the atomic weight of each. A decided confirmation was given to this guess by Dalton's discovery that when different gases combine, it is always in proportions expressed by whole numbers. This could most readily be explained by the theory that these gases were made up of indivisible particles called atoms, whose union conditioned the proportion between the uniting masses of gases. This atomic theory was somewhat combated by a few chemists, even Sir Humphry Davy and Mr. Wollaston for a brief time opposing it. It soon, however, made its way, and its general principles have been received by all chemists; for nearly a century it has dominated, or rather has been the foundation of, chemical theory.

According to this theory, all matter is composed of some seventy different kinds of atoms, each possessed of independent and permanent properties. Lists of the atomic weights of the commoner elements were rapidly published, the most notable being that of Berzelius, in 1815.

The fact of so many different kinds of ultimate particles of matter was naturally a great blow to those who believed in its unity. Very early there was speculation as to whether any connection existed between the different kinds of atoms. The first step in this direction was what is known as Prout's hypothesis, which was first enunciated

in 1815, with no author's signature, in Thomson's 'Annals of Philosophy.' Prout had noticed that the atomic weights of many of the lighter elements seemed to be exact multiples of that of hydrogen; hence he made the suggestion that all the different atoms may be merely aggregations of the simple hydrogen atom, and that this hydrogen atom is really the primitive element from which all other substances are made.

This was the first attempt to determine a relation between the apparently different kinds of matter, and it was more than eighty years before the advocates of the theory were finally forced to abandon it. There have been few laws in chemistry, and certainly no false hypotheses, which have given rise to so much investigation as that which has been occasioned by Prout's hypothesis. That it could have so long retained adherents among chemists, many of them men of great prominence, is due to the fact that it seems on its face to be true. When it was first published, a very considerable number of the atomic weights were approximately multiples of the weight of the hydrogen atom, far more than could be accounted for by chance. It seemed reasonable to believe that, with the meager facilities for accurate work at that day, the atoms of the few other elements would prove, when they should be accurately determined, to be also exact multiples of the hydrogen atom. This view was held by many chemists until a Belgian chemist, Jean Servais Stas, undertook to determine the atomic weight of a few of the elements with an accuracy far greater than had been known up to that time. Prout's hypothesis had been sustained by rounding off the decimals to whole numbers; Stas, before he began this work an earnest believer in the hypothesis, endeavored to determine at least one place of decimals so accurately that it could not hereafter be neglected, and his work is one of the classics of chemistry. He proved clearly that the atoms of several elements, at least, could not be multiples of that of hydrogen. Some of the supporters of the hypothesis then assumed that it was not the hydrogen atom, but a half of it, or some other fraction, which is the original matter, from which all other atoms are derived. The hypothesis may be said to have finally ended its long career when Professor Morley, of Adelbert College, showed that there is no simple ratio between the atomic weights of oxygen and hydrogen; that, instead of being 16:1, it is 15.879:1. For accuracy Professor Morley's work may be justly compared with that of Stas, but in conception of experiment and in difficulty of execution it far surpasses that of the Belgian chemist.

But while it may be considered as absolutely proved that a large share of the atoms have weights which are *not* exact multiples of that of hydrogen, yet it remains true that many of those which have been determined with the greatest degree of certainty do approach with

wonderful closeness to exact multiples. This is shown by the following table, taken from the report of the Committee on Atomic Weights of the American Chemical Society for 1899:

Arsenic	75.0	Lead	206.92	Phosphorus	31.0
Boron	11.0	Lithium	7.03	Rhodium	103.0
Bromin	79.95	Manganese	55.0	Silver	107.92
Carbon	12.0	Mercury	200.0	Sodium	23.05
Cerium	139.0	Nitrogen	14.04	Sulfur	32.07
Cobalt	59.00	Osmium	191.0	Tin	119.0
Gallium	70.0	Oxygen	16.00	Yttrium	89.0
Iron	56.0	Palladium	107.0		

In addition to these at least nine others have atomic weights differing not more than 0.1 from whole numbers. By the law of probabilities this close approach to whole numbers cannot be the result of chance, but no satisfactory explanation has as yet been offered.

Prout's hypothesis was not unique in concerning itself with an effort to show a unity of matter. Very early there was noticed a connection between the atomic weights and the properties of certain groups of elements. Attention was first called to this by Professor Döbereiner, of Jena, and an account was given of it in print in 1816, just after the first enunciation of Prout's hypothesis. Döbereiner noticed that the equivalent weight of strontium was 50, while the values then accepted for calcium and barium were respectively 27.5 and 72.5. Fifty is the mean of 27.5 and 72.5, and the properties of strontium may be looked upon as being an average of those of calcium and barium. It was soon clear, however, that strontium was as much entitled to recognition as an element as calcium or barium. Hence it appeared that there was a numerical relation between the weights of the atoms of these three elements, barium, strontium and calcium, which corresponded to both the chemical and the physical properties of the elements. Several other similar groups of three were discovered by Döbereiner, and this, which was really the earliest germ of the Periodic Law, became known as Döbereiner's law of triads. It is of especial interest, as having enabled its author to predict the atomic weight of bromin, which was later confirmed by experimental investigation. In this respect it anticipated the Periodic Law, and may be said to represent a phase of this later and greater generalization.

Little attention was attracted by the speculations of Döbereiner, and a quarter of a century or so later the subject was taken up anew by the great French chemist, Dumas. He developed to some extent the law of triads, though he made little actual advance beyond the point attained by Döbereiner. Dumas's work was, however, widely noticed, and proved very stimulating to the chemists of his day. It

is interesting to read the comments of Faraday: "This circumstance (the numerical relations between chlorin, bromin and iodin) has been made the basis of some beautiful speculations by M. Dumas, speculations which have scarcely yet assumed the consistence of a theory, and which are at the present time to be ranged among the poetic day-dreams of a philosopher; to be regarded as some of the poetic illuminations of the mental horizon, which possibly may be the harbinger of a new law. . . . We seem here to have the dawning of a new light, indicative of the mutual convertibility of certain groups of elements, although under conditions which as yet are hidden from our scrutiny." In the succeeding decade we find many chemists speculating in a similar way upon the connection which seemed to subsist between the different elements.

The two chemists whose names are associated with the dawn of the Periodic Law are De Chancourtois and Newlands. De Chancourtois arranged the elements in the order of their atomic weights in a helix inscribed upon a vertical cylinder; this he called a 'telluric screw,' and although there were many inaccuracies, as a whole it approached a form in which the Periodic Law is to-day sometimes represented. The ideas of De Chancourtois were by no means free from considerable haze, as, for example, when he states that 'the properties of bodies are the properties of numbers.' This may well be interpreted in the light of the Periodic Law, which affirms that the properties of elements are functions of their atomic weights. Even the important idea of periodicity is not overlooked by De Chancourtois, but the speculations of this ingenious French engineer and geologist had practically no effect upon the chemical thought of that day; indeed, his articles were almost unnoticed and were resurrected only after they had slumbered for nearly thirty years in obscurity.

Somewhat otherwise was it with the work of Newlands, which began to appear in 1863, just a year later than that of his French contemporary. His work was, however, wholly independent of that of De Chancourtois. His first paper was chiefly concerned with the development of numerical relations between the atomic weights, following out the ideas early expressed in Döbereiner's triads. He enlarged this so as to include more than three elements in a group. For example, not only was sodium the middle member, with mean properties, of the triad, lithium, sodium, potassium; but rubidium also belonged to this group, because two of potassium plus one of lithium gives the atomic weight of rubidium. A year later he announced his law of octaves, which is generally looked upon as a forerunner of the Periodic Law. Here he arranged the elements in the order of their atomic weights and showed that "elements having consecutive numbers frequently either belong to the same group or occupy similar positions in different

groups." "The difference between the number of the lowest member of a group and that immediately above it is seven; in other words, the eighth element starting from a given one, is a kind of repetition of the first, like the eighth note of an octave in music." While this regularity appeared in the case of the elements of low atomic weight, it failed when applied to many of those elements which have a higher weight, and also in the case of iron, cobalt and nickel. These three metals seem to break in upon the octaves, and must be left out of account before the law of octaves can be used. This irregularity Newlands noticed, enunciating his law in the words: "The numbers of analogous elements, *when not consecutive*, differ by seven, or by some multiple of seven." By 'number' he means merely the number of the element when all are arranged in a series in the order of their atomic weight. There was thus here, as in the work of De Chancourtois, the vision of a certain periodicity in the actual arrangement of the elements, and the recognition of the fact that in some way there is a connection between the properties of an element and its atomic weight. But there seemed to be no suspicion that all the properties of an element are a function, much less that they are a periodic function of its atomic weight.

It may seem strange to us that the work of these two pioneers should have been received with almost complete indifference by chemists. This results in part, at least, as has been pointed out by Mendeléeff, from the too-limited application of Newlands's law. Relations were brought out between little groups of elements, like Döbereiner's triads, but comparisons were not made between dissimilar elements, and even the groups made up by taking the seventh elements often contained those which were far from being similar in their properties. Thus we find the first, and hence analogous, elements of his eight octaves as follows: Hydrogen, fluorin, chlorin, cobalt-nickel, bromin, palladium, tellurium, platinum-iridium. Such a grouping as this could hardly be expected to appeal strongly to chemists, especially as iodin, an element which obviously belongs with fluorin, chlorin and bromin, is relegated to the seventh group of elements. The lack of enthusiasm on the part of chemists at the reception of Newlands's work may be judged from an incident. When his paper was read at the meeting of the Chemical Society, one of the members present asked of Professor Newlands whether he had ever tried arranging the elements according to the order of their initial letters.

The Periodic Law in its present form was first enunciated by Professor Dmitri Mendeléeff, in 1869, in a paper read before the Russian Physico-Chemical Society. It is true that five years earlier Lothair Meyer had published in the first edition of his 'Modern Theory of Chemistry' a list of the elements arranged according to atomic weights,

somewhat after the method of Newlands, but this table could be considered in no sense an advance upon the table of his English contemporary. It was not so much a periodic table as a summary of the grouping of the elements in more or less natural groups. Meyer, indeed, made his earlier table the basis of his later work, but these subsequent amendments to the table were made after the publication of Mendeléeff's first table and show clearly the influence of his work.

In his first paper, Mendeléeff gives several different arrangements of the elements, all, however, embodying the same principles. The principal table, of which the others are variants, shows many errors and crudities, but the underlying principles of the Periodic Law, as to-day recognized, are clearly apparent. This table is as follows:

MENDELÉEFF'S FIRST TABLE. 1869.

			Ti 50	Zr.... 90	? 180
			V.......51	Nb... 94	Ta ...182
			Cr 52	Mo .. 96	W....186
			Mn.....55	Rh...104.4	Pt....197.4
			Fe 56	Ru ..104.4	Ir....198
			Ni, Co..59	Pd...106.6	Os...199
H......1			Cu......63.4	Ag...108	Hg.. 200
	Be.... 9.4	Mg...24	Zn......65.2	Cd...112	
	B.....11	Al.....27.4	? 68	Ur...116	Au ..197
	C.....12	Si.....28	? 70	Sn ...118	
	N.....14	P.....31	As..... 75	Sb...122	Bi....210
	O.....16	S......32	Se 79.4	Te....128 ?	
	F.....19	Cl....35.5	Br......80	I.....127	
	Na.....23	K.....39	Rb.....85.4	Cs...133	Tl....204
		Ca....40	Sr......87.6	Ba...137	Pb...207
		? 45	Ce 92		
		? Er ..56	La 94		
		? Y ..60	Di......95		
		? In...75.6	Th.....118		

The resemblance to the modern tables comes out yet more strongly when we examine Mendeléeff's horizontal table, which was published in the same paper, and in which the doubtful elements and those whose position was not clear were omitted:

MENDELÉEFF'S HORIZONTAL TABLE. 1868.

Li	Na	K	Cu	Rb	Ag	Cs	..	Tl
Be	Mg	Ca	Zn	Sr	Cd	Ba	..	Pb
B	Al	..	..	..	Ur	..	..	B
C	Si	Ti	..	Zr	Sn	..	..	..
N	P	V	As	Nb	Sb	..	Ta	..
O	S	..	Se	..	Te	..	W	..
F	Cl	.	Br	..	I	..	..	..

In the first table the elements are arranged in vertical columns, in the order of their atomic weights. They fall in a way into groups of seven, as in Newlands's octaves, but after the first two octaves there

are quite a number of elements before the next group of seven is reached, and the same is true in each succeeding column. The next year, 1870, Meyer published a table in which he brought these outside elements into something of order by pointing out the existence of a double periodicity after the first two octaves have been passed, and showing that the alternate periods resemble each other closely. This was brought out with greater clearness in the revised table which Mendeléeff published in 1871. The skill of the author of this table is apparent when we consider that it is, with few additions, the generally accepted table in use at the present day. This table, which is given on the following page, when compared with that of two years before, shows how great had been the development.

One well-recognized test of the truth of any theory is its use in prediction. In this table Mendeléeff did not hesitate to make certain changes in the generally received atomic weights, in order to bring facts into conformity with his table. His was not the position of the ancient philosopher who would have all phenomena bend to his preconceived theory, and if the facts failed to yield, so much the worse for the facts. Mendeléeff had confidence that this Periodic Law was the expression of a great truth of nature, and so firm was his confidence that he could not but believe that when the phenomena did not agree, it was from imperfect observation and interpretation of the facts. A good instance of this is seen in the case of the metals of the platinum group. As far as observation had gone, osmium had the largest atomic weight of these metals, followed by iridium and platinum, of equal weight, and all these metals were lighter than gold. According to the Periodic Law the reverse should be the case. Mendeléeff affirmed that the discrepancy in this case was probably due to the fact that the atomic weights of these metals had not been determined with an accuracy commensurate with the work of the table. It is an interesting confirmation that some years later, Seubert took up this atomic weight problem, and found that the views of Mendeléeff were correct. Gold has the highest atomic weight of these elements, platinum the next highest, iridium follows and osmium comes lowest of all, its previously determined weight having been seven or eight units too high.

Along the line of predictions a still more remarkable use of the table appeared in connection with the vacant spaces. There were many places in the table where elements might be expected, which were, however, then unknown. Could the table stand the test of actually predicting the existence of an unknown element? Mendeléeff did not think this too great a strain to put upon his work, and he ventured not merely to predict that elements might be expected with atomic weights of 44, 68 and 72, but he was even bold enough to describe these elements under the names of eka-boron, eka-aluminum and eka-

MENDELEEFF'S TABLE OF 1871.

SERIES.	GROUP I. R_2O.	GROUP II. RO.	GROUP III. R_2O_3.	GROUP IV. RH_4. RO_2.	GROUP V. RH_3. R_2O_5.	GROUP VI. RH_2. RO_3.	GROUP VII. R H. R_2O_7.	GROUP VIII. RO_4.
1.........	H=1							
2.........	Li=7	Be=9.4	B=11	C=12	N=14	O=16	F=19	
3.........	Na=23	Mg=24	Al=27.3	Si=28	P=31	S=32	Cl=35.5	
4.........	K=39	Ca=40	— = 44	Ti=48	V=51	Cr=52	Mn=55	Fe=56, Co=59 Ni=59, Cu=63
5.........	(Cu=63)	Zn=65	— = 68	— = 72	As=75	Se=78	Br=80	
6.........	Rb=85	Sr=87	? Y=88	Zr=90	Nb=94	Mo=96	— = 100	Ru=104, Pd=104 Pd=106, Ag=108
7.........	(Ag=108)	Cd=112	In=113	Sn=118	Sb=122	Te=125	I=127	
8.........	Cs=133	Ba=137	? Di=138	? Ce=140	......	......	......	......
9.........	......	......	......	......	......	......	......	....
10.........	......	......	? Er=178	? La=180	Ta=182	W=134	......	Os=195, Ir=197 Pt=198, Au=199
11.........	(Au=199)	Hg=200	Tl=204	Pb=207	Bi=208	......	......	
12.........	......	......	......	Th=231	......	U=240	......	......

silicon, in several cases going into considerable detail as to the properties of the elements and their compounds. It was in 1875 that the first of these predictions was fulfilled in the discovery of gallium by Lecoq de Boisbaudran. This metal fell in the place which Mendeléeff had given to eka-aluminum, and its specific gravity is 5.9, while 5.8 was the figure which had been foretold. Four years later Nilson discovered eka-boron, and gave to it the name scandium. In 1885 a new silver mineral, argyrodite, was found in the Freiberg mines, and every analysis made of it showed a discrepancy of six or seven per cent. This soon led to the recognition by the analyst, Clemens Winkler, of the presence of a new element, and it further appeared that this new element was Mendeléeff's eka-silicon. Not to be outdone by the French and Swedish chemists, Winkler patriotically called the new metal germanium. It is worth while to show side by side, a few of the predictions of the properties of eka-silicon, published by Mendeléeff in 1872, and the actual properties of germanium, as experimentally determined by Winkler in 1886:

Eka-Silicon. Symbol, Es.	Germanium. Symbol, Ge.
ELEMENT.	
Atomic weight, 72.	Atomic weight, 72.3.
Specific gravity, 5.5.	Specific gravity, 5.469 at 20°.
OXID.	
Formula, EsO_2.	Formula, GeO_2.
Specific gravity, 4.7.	Specific gravity, 4.703 at 18°.
CHLORID.	
Formula, $EsCl_4$.	Formula, $GeCl_4$.
Liquid, boiling a little below 100°.	Liquid, boiling at 86°.
Specific gravity, 1.9 at 0°.	Specific gravity, 1.887 at 18°.
METALLO-ORGANIC COMPOUND.	
Formula, $Es(C_2H_5)_4$.	Formula, $Ge(C_2H_5)_4$.
Liquid, boiling point, 160°.	Liquid, boiling point, 160°.
Specific gravity, 0.96.	Specific gravity, slightly less than water (which is 1.0).

So close is this agreement that it is difficult to realize that Mendeléeff's forecasts were put in print more than a decade before the element had ever been handled by man.

Since the corrected form of Mendeléeff's table was published in 1871, there has been no end to the speculation upon the subject, and dozens of tables, emphasizing different relations of the elements, have been proposed. Few of these have equaled that of the Russian chemist in simplicity or have as few obscure points. One of these tables, suggested a few years ago by Dr. F. P. Venable, of the University of North Carolina, may be noticed as presenting some decided advantages over that of Mendeléeff:

VENABLE'S TABLE. 1895. (SLIGHTLY MODIFIED.)

H														He ?		
Li		Gl		B		C		N		O		F		Ne ?		
Na		Mg		Al		Si		P		S		Cl		Ar ?		
K	Cu	Ca	Zn	Sc	Ga	Ti	Ge	V	As	Cr	Se	Mn	Br	Fe	Co	Ni
Rb	Ag	Sr	Cd	Y	In	Zr	Sn	Cb	Sb	Mo	Te	▬	I	Ru	Rh	Pd
Cs	†	Ba	†	La	†	Ce	†	*	†	*	†	*	†	*	*	*
*	Au	*	Hg	*	Tl	*	Pb	Ta	Bi	W	†	*	†	Os	Ir	Pt
*		*		*		Th		*		U						

* † Possible elements, now unknown. ▬ Eka-Manganese.

Most tables present a difficulty in that they place sodium in the group or sub-group with copper, silver and gold, while it would most naturally fall in the group with lithium, potassium, rubidium and cesium. So magnesium would be placed according to its properties, not so closely with zinc and cadmium, as with glucinum, calcium, strontium and barium. Fluorin belongs rather with chlorin, bromin and iodin than with manganese, the metal with which it is associated in most tables. So oxygen belongs with sulfur, selenium and tellurium, rather than with chromium and molybdenum. This is remedied in Venable's table. The first element in any group the author calls the group element or bridge element; for it often possesses properties which ally it to the elements of the next groups. The second element he calls the type element, and in this is, as it were, shadowed forth the character of the succeeding elements of the same group. From this point on, the group is divided into two series, one more and the other less electro-positive or negative, as the case may be. The first three groups are for the most part made up of electro-positive elements, while those of the fifth, sixth and seventh groups are relatively electro-negative. Now in the former the elements of the more positive series resemble the type element of their group more strongly than those of the less positive series. In the relatively negative fifth, sixth and seventh groups, the reverse is the case. This grouping thus places sodium with potassium and chlorin with bromin. In the fourth group,

which lies between the extremes, the type element, silicon, foreshadows the members of its two series to an approximately equal extent.

As the tables which graphically portray the Periodic Law stand to-day, there is much which still remains to be cleared up. At the very outset we are met by the fact that we cannot tell in which group as familiar an element as hydrogen ought to be placed. It generally receives the first place in group one, but to some extent at least this position is based upon a misapprehension. Some years ago Pictet, of Geneva, was engaged in that work on the condensation and liquefaction of gases, which has rendered his name famous. On compressing hydrogen at a very low temperature, he obtained, on suddenly reducing the pressure, some heavy, steel-blue drops, much resembling mercury. This was erroneously supposed to be hydrogen in the liquid form. As a result, it seemed only natural to classify hydrogen with the metallic elements of the first group. Not only have later investigations shown that these drops were not liquid hydrogen, but quite recently Dewar has actually obtained this substance, which proves to be a colorless, limpid liquid, with the extraordinarily low specific gravity of about 0.07. As far then as physical properties go, there is no justification in classifying hydrogen with the metals of group one. Chemically, however, hydrogen is like the metals, electro-positive, though very weakly so, and it is possible that its position in the first group is less awkward than would be any other.

Of the other elements in the table with atomic weight below 100, all seem fairly well placed, though we have not as much knowledge of scandium as we could wish, and there is a difficulty that we shall soon notice in the eighth group. The first apparent blank space in the table is for an element with atomic weight of about 100. Such an element would be known as eka-manganese, and would possess properties which would to a considerable extent resemble those of manganese, but perhaps more closely those of ruthenium. Beyond this in the table we find many gaps, partly from the inadequacy of our chemical knowledge and partly from the likelihood that there exist rare elements which have not yet been discovered. Such elements probably occur in extremely small quantities, and may, for many years, perhaps forever, elude chemists. It seems improbable that there are undiscovered elements which exist in more than very small quantities; this is the testimony, not only of the chemical laboratory, but also of the spectroscope, that instrument which reveals to us the composition, not merely of substances in the laboratory, but also that of the sun and of the distant stars. From barium to tantalum few elements are known to which a definite place can be assigned, but here there is an indefinitely large number of what Crookes has called meta-elements, the rare earths. Their rela-

tion to the table is as yet hardly more than speculative, and they have been likened to the asteroids of the solar system.

From tantalum to bismuth the table is very regular, except that the element of the manganese series, which would have an atomic weight of about 188, is unknown, as we have already seen is the case with eka-manganese. Above bismuth, with its weight of 208, we know but two elements, thorium, 231, and uranium, 240. Since the discovery of the Röntgen rays, great interest has been excited by different kinds of rays which, though they may not be visible to the naked eye, are nevertheless capable of affecting the photographic plate. The only elements, as far as yet known, which yield such rays are these two of extraordinarily high atomic weight, thorium and uranium. Closely connected with this phenomenon is that of giving off luminous rays when not exposed to light. It has recently been discovered that while uranium can give off comparatively feeble rays, there is contained in the principal uranium mineral, pitchblende, matter which is much more active than uranium. Further investigation seems to show that there are at least three such substances present in pitchblende; which have been named radium (from the rays it gives off), polonium (from its discoverer's native land), and actinium (from its ratio-activity). Of these radium alone has been studied at all extensively, and even its claim to be called a chemical element is by no means established. It strongly resembles barium, but it gives off rays easily visible in the dark, continuing to shine indefinitely. There is much doubt as to whether it be not really a peculiar form of barium, but recent determinations of its atomic weight, in a condition only partially purified, indicate that it has a higher atomic weight than barium, and that it may in this respect resemble thorium and uranium.

It may seem rather remarkable that, inasmuch as Döbereiner had brought out the resemblances between elements of the same group in his triads, nearly half a century should have elapsed before the essential features of the Periodic Law were discovered. This is due, chiefly at least, to three causes. First, there would have been many more gaps in the table then than now, so many new elements having been discovered since that day; second, the atomic weights of the elements were then so imperfectly known that, using the weights then accepted, it is impossible to construct a periodic table; the third great difficulty lay in the fact that nine very important elements refused to be reduced to order, and finally were excluded and relegated to an outlying group of unique properties. These nine elements are iron, cobalt, nickel and the so-called platinum metals, platinum, palladium, iridium, rhodium, osmium and ruthenium. As a matter of fact, these nine metals cannot be brought into any of the seven regular groups, but must be placed by themselves in a single group of three series, or in three groups. This

eighth group proves to be transitional between group seven and group one; iron, cobalt and nickel make a direct gradation from manganese to copper; ruthenium, rhodium and palladium, from molybdenum to silver; osmium, iridium and platinum, from tungsten to gold. Not only do these three triplets stand between these other elements in atomic weight, but their properties also show a similar gradation.

While now we have these transitional elements, the question might very naturally arise whether there are similar transitional elements from fluorin to sodium and from chlorin to potassium. The case here is, however, somewhat different from the former one. Manganese and copper are both metals, and not so widely separated in properties; the transitional elements, iron, cobalt and nickel, partake of the nature of both extremes, and the transition seems a natural one. Hardly any elements can be more unlike than fluorin and sodium, or chlorin and potassium. Chlorin is very electro-negative, potassium as strongly electro-positive. A transitional element would thus probably be inert, that is, lacking in both electro-positiveness and in electro-negativeness, and up to a few years ago such an element could hardly have been conceived of. At that time Lord Rayleigh was engaged in determining with all possible accuracy the density of nitrogen. In this work he prepared nitrogen by several different methods. Some specimens were obtained by the decomposition of chemical compounds, such as urea and ammonium nitrite, others from the air by removing the oxygen. To his surprise, Lord Rayleigh found that in every case the nitrogen obtained from the atmosphere was slightly heavier than that prepared from chemical compounds. In searching for the cause of this difference, Lord Rayleigh and Professor Ramsay, who had been associated with him in this work, found that there is present in the atmosphere a new gas, much like nitrogen in its properties, whose existence, although it is present to the extent of nearly one per cent, had been unsuspected. This gas, christened argon from its inertness, is nearly three times as heavy as nitrogen, and it is this that increases the weight of atmospheric nitrogen slightly above the weight of pure nitrogen, obtained from chemical compounds. Stimulated by this discovery it was not long before Ramsay had isolated from the atmosphere at least two other gases, both characterized by an inertness similar to that of argon. These are helium, whose spectrum had long been known from the fact that this gas is plentiful in the corona of the sun, and neon. It is probable that there are several other similar gases in the atmosphere, and one, xenon, has been recently isolated by Ramsay. It is not uninteresting to note that argon had been in the hands of chemists from the time of Cavendish down, but all had supposed it to be nitrogen. Under the influence of the electric spark oxygen and nitrogen may be made to combine with each other. In Cavendish's experiment

the spark was passed through the air, which consists chiefly of a mixture of nitrogen and oxygen, and the resultant oxid of nitrogen was absorbed in caustic potash. More oxygen was added from time to time until the last of the nitrogen was used up. Now Cavendish noticed, as have many chemists since his day, that it was always impossible to remove all the nitrogen: in every case about one per cent. of gas remained. There is no record that any one ever suspected that this residue was not nitrogen; such is, however, the fact, and the gases, argon, helium and, perhaps, others are present. These can be best recognized by passing an electric spark through the rarefied gas and examining the spectrum. It seems now very strange to us that an element so abundant that an ordinary sized room contains no less than a thousand liters should have so long escaped discovery. The reason is not far to seek. Argon and its congeners are distinguished by a most remarkable exhibition of properties, in that they have apparently no chemical affinity, and no compounds of them are known. From this fact it has been argued by some that these gases cannot be considered chemical elements, for all elements hitherto known do form compounds with other elements. It is, however, a curious fact that in the periodic table we find, in the eighth group, place for several just such elements, as we have seen, without affinity, and neither positive nor negative in electrochemical character. It may well be that helium, neon, argon and xenon belong in these vacant spaces.

If this be the case, there is still a difficulty which confronts us, and this is that argon possesses an atomic weight slightly higher than the next element in order, potassium, instead of lower. This would not, however, be a unique instance of such a difficulty in the table. It was formerly thought that the two metals, nickel and cobalt, had identical atomic weights, and though the salts of nickel are generally green, and those of cobalt red, in other respects these metals and their compounds are very much alike. After the discovery of the Periodic Law, when it was seen that cobalt belonged in the second series of the eighth group and nickel in the third, it was supposed that further study would necessarily show nickel to have an appreciably higher atomic weight than cobalt. We have already seen that in this same group, before the appearance of the periodic table, the accepted atomic weights of osmium, iridium and platinum were incorrect, and it was the fact of their mis-arrangement in the table which caused Seubert to revise their weights. Very much labor has been spent upon the revision of the atomic weights of nickel and cobalt. Gerhardt Krüss supposed he had found a new and heavier metal, hitherto unknown, in ordinary cobalt, and that this caused the atomic weight to be estimated too high. He called the new metal gnomium, but it was soon shown that gnomium has no real existence. The more accurate the

determinations, the more probable it seemed that in reality cobalt, and not nickel, as demanded by theory, has the greater weight. The whole subject has been very carefully investigated in the last few years by Prof. Theodore W. Richards at Harvard University, and there seems now to be no doubt but that Nature has unexpectedly and inexplicably reversed the position of these elements.

Another instance that seems to be of the same nature is that as far as the most accurate determinations go, tellurium has an atomic weight greater than that of iodin, instead of less. At present it is impossible to explain these abnormalities, but they assure us of the possibility that argon may have a higher atomic weight than potassium, and yet belong to the eighth group.

What now is the present position of the philosophy of matter from the light thrown upon it by the Periodic Law? In the first place, drawing our deduction from the marvelously accurate determinations of the relative weights of the atoms of the different elements, to which chemists have been incited by the Periodic Law, it may be considered as absolutely settled that the elements are not groups of hydrogen atoms, nor are they composed of half or quarter hydrogen atoms. As enunciated by Prout, the hypothesis which goes by his name may be considered as finally proved untenable; the atomic weights are not multiples of the weight of the hydrogen atom, nor any simple fraction thereof. But while this is the case, it is perfectly clear from the Periodic Law that the properties of an atom are a periodic function of its atomic weight. It would seem that this can be true only if the material of which all atoms are made is the same. This does not necessarily mean that there is but one kind of matter, and that all atoms are merely different quantities of this 'urstoff.' There may be several kinds of matter, and different kinds of atoms may represent varying proportions of a few constituents.

There have been many attempts to reduce the Periodic Law to mathematics, in order to find a numerical value for the function which expresses the relation between atomic weight and an element's place in the series. Such efforts have been thus far wholly unsuccessful. It is by no means impossible that such relations will be found in the future, but at present the atomic weights of comparatively few elements have been determined with great accuracy. When this work has been extended to a greater number of elements, and when the position of the rare elements and of the inert atmospheric gases has been definitely settled, we may hope for more light upon the principles underlying the Periodic Law.

At present this law occupies much the same position as two other great generalizations of natural science. The fact of gravitation was long ago discovered. The laws by which it acts are well known, and

yet the cause of gravitation is even to-day a mere speculation, and no link has yet been discovered to connect the three phases of attraction, gravitation between masses, cohesion and adhesion between molecules, and chemical affinity between atoms. The fact of evolution is universally recognized, much is known and more is a matter of speculation as to how and why evolution has taken place, but why an organism tends to resemble its progenitor and why it tends to vary are as unknown as before the days of Darwin and Wallace. So the Periodic Law is, after all, a mere statement of fact. But it is a statement which has already exerted upon chemistry an influence as great as that of gravitation and evolution in their respective fields, and is destined more and more to become the very foundation of chemistry. We may hope and confidently expect that it will eventually lead us to the real constitution of matter.

A PLEA FOR PURE SCIENCE.*

BY THE LATE PROFESSOR HENRY A. ROWLAND,

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THE question is sometimes asked us as to the time of year we like the best. To my mind, the spring is the most delightful; for nature then recovers from the apathy of winter, and stirs herself to renewed life. The leaves grow, and the buds open, with a suggestion of vigor delightful to behold; and we revel in this ever-renewed life of nature. But this cannot always last. The leaves reach their limit; the buds open to the full, and pass away. Then we begin to ask ourselves whether all this display has been in vain, or whether it has led to a bountiful harvest.

So this magnificent country of ours has rivalled the vigor of spring in its growth. Forests have been levelled, and cities built, and a large and powerful nation has been created on the face of the earth. We are proud of our advancement. We are proud of such cities as this, founded in a day upon a spot over which, but a few years since, the red man hunted the buffalo. But we must remember that this is only the spring of our country. Our glance must not be backward; for however beautiful leaves and blossoms are, and however marvelous their rapid increase, they are but leaves and blossoms after all. Rather should we look forward to discover what will be the outcome of all this, and what the chance of harvest. For if we do this in time, we may discover the worm which threatens the ripe fruit, or the barren spot where the harvest is withering for want of water.

I am required to address the so-called physical section of this association. Fain would I speak pleasant words to you on this subject; fain would I recount to you the progress made in this subject by my countrymen and their noble efforts to understand the order of the universe. But I go out to gather the grain ripe to the harvest, and I find only tares. Here and there a noble head of grain rises above the weeds; but so few are they, that I find the majority of my countrymen

* This address by Prof. H. A. Rowland, whose recent death all men of science deplore, was given before the Section of Physics of the American Association for the Advancement of Science in 1883. It is here republished as a tribute to his memory, demonstrating as it does his keen intellect and strong personality. While the state of American science in 1883 was scarcely as backward as might be supposed from reading this address, there has certainly been a remarkable advance in the past eighteen years. In this advance Rowland was one of the great leaders.—EDITOR.

know them not, but think that they have a waving harvest, while it is only one of weeds after all. American science* is a thing of the future, and not of the present or past; and the proper course of one in my position is to consider what must be done to create a science of physics in this country rather than to call telegraphs, electric lights and such conveniences by the name of science. I do not wish to underrate the value of all these things: the progress of the world depends on them, and he is to be honored who cultivates them successfully. So also the cook who invents a new and palatable dish for the table benefits the world to a certain degree; yet we do not dignify him by the name of a chemist. And yet it is not an uncommon thing, especially in American newspapers, to have the *applications* of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications, had his mind possessed the necessary element of vulgarity. I have often been asked, which was the more important to the world, pure or applied science. To have the applications of a science, the science itself must exist. Should we stop its progress, and attend only to its applications, we should soon degenerate into a people like the Chinese, who have made no progress for generations, because they have been satisfied with the applications of science, and have never sought for reasons in what they have done. The reasons constitute pure science. They have known the application of gunpowder for centuries; and yet the reasons for its peculiar action, if sought in the proper manner, would have developed the science of chemistry, and even of physics, with all their numerous applications. By contenting themselves with the fact that gunpowder will explode, and seeking no farther, they have fallen behind in the progress of the world; and we now regard this oldest and most numerous of nations as only barbarians. And yet our own country is in this same state. But we have done better; for we have taken the science of the old world, and applied it to all our uses, accepting it like the rain of heaven, without asking whence it came, or even acknowledging the debt of gratitude we owe to the great and unselfish workers who have given it to us. And, like the rain of heaven, this pure science has fallen upon our country, and made it great and rich and strong.

To a civilized nation of the present day, the applications of science are a necessity; and our country has hitherto succeeded in this line, only for the reason that there are certain countries in the world where

* In using the word 'science,' I refer to physical science, as I know nothing of natural science. Probably my remarks will, however, apply to both, but I do not know.

pure science has been and is cultivated, and where the study of nature is considered a noble pursuit. But such countries are rare, and those who wish to pursue pure science in our own country must be prepared to face public opinion in a manner which requires much moral courage. They must be prepared to be looked down upon by every successful inventor whose shallow mind imagines that the only pursuit of mankind is wealth, and that he who obtains most has best succeeded in this world. Everybody can comprehend a million of money; but how few can comprehend any advance in scientific theory, especially in its more abstruse portions! And this, I believe, is one of the causes of the small number of persons who have ever devoted themselves to work of the higher order in any human pursuit. Man is a gregarious animal, and depends very much, for his happiness, on the sympathy of those around him; and it is rare to find one with the courage to pursue his own ideals in spite of his surroundings. In times past, men were more isolated than at present, and each came in contact with a fewer number of people. Hence that time constitutes the period when the great sculptures, paintings and poems were produced. Each man's mind was comparatively free to follow its own ideals, and the results were the great and unique works of the ancient masters. To-day the railroad and the telegraph, the books and newspapers, have united each individual man with the rest of the world: instead of his mind being an individual, a thing apart by itself, and unique, it has become so influenced by the outer world, and so dependent upon it, that it has lost its originality to a great extent. The man who in times past would naturally have been in the lowest depths of poverty, mentally and physically, to-day measures tape behind a counter, and with lordly air advises the naturally born genius how he may best bring his outward appearance down to a level with his own. A new idea he never had, but he can at least cover his mental nakedness with ideas imbibed from others. So the genius of the past soon perceives that his higher ideas are too high to be appreciated by the world: his mind is clipped down to the standard form; every natural offshoot upward is repressed, until the man is no higher than his fellows. Hence the world, through the abundance of its intercourse, is reduced to a level. What was formerly a grand and magnificent landscape, with mountains ascending above the clouds, and depths whose gloom we cannot now appreciate, has become serene and peaceful. The depths have been filled, and the heights levelled, and the wavy harvests and smoky factories cover the landscape.

As far as the average man is concerned, the change is for the better. The average life of man is far pleasanter, and his mental condition better than before. But we miss the vigor imparted by the mountains, We are tired of mediocrity, the curse of our country. We are tired of

seeing our artists reduced to hirelings, and imploring Congress to protect them against foreign competition. We are tired of seeing our countrymen take their science from abroad, and boast that they here convert it into wealth. We are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science; or sitting inactive while the whole world is open to investigation; lingering by the wayside while the problem of the universe remains unsolved. We wish for something higher and nobler in this country of mediocrity, for a mountain to relieve the landscape of its monotony. We are surrounded with mysteries, and have been created with minds to enjoy and reason to aid in the unfolding of such mysteries. Nature calls to us to study her, and our better feelings urge us in the same direction.

For generations there have been some few students of science who have esteemed the study of nature the most noble of pursuits. Some have been wealthy, and some poor; but they have all had one thing in common—the love of nature and its laws. To these few men the world owes all the progress due to applied science, and yet very few ever received any payment in this world for their labors.

Faraday, the great discoverer of the principle on which all machines for electric lighting, electric railways and the transmission of power must rest, died a poor man, although others and the whole world have been enriched by his discoveries. And such must be the fate of the followers in his footsteps for some time to come.

But there will be those in the future who will study nature from pure love, and for them higher prizes than any yet obtained are waiting. We have but yet commenced our pursuit of science, and stand upon the threshold wondering what there is within. We explain the motion of the planets by the law of gravitation; but who will explain how two bodies, millions of miles apart, tend to go toward each other with a certain force?

We now weigh and measure electricity and electric currents with as much ease as ordinary matter, yet have we made any approach to an explanation of the phenomenon of electricity? Light is an undulatory motion, and yet do we know what it is that undulates? Heat is motion, yet do we know what it is that moves? Ordinary matter is a common substance, and yet who shall fathom the mystery of its internal constitution?

There is room for all in the work, and the race has but commenced. The problems are not to be solved in a moment, but need the best work of the best minds, for an indefinite time.

Shall our country be contented to stand by, while other countries lead in the race? Shall we always grovel in the dust, and pick up the crumbs which fall from the rich man's table, considering ourselves richer than he because we have more crumbs, while we forget that he

has the cake, which is the source of all crumbs? Shall we be swine, to whom the corn and husks are of more value than the pearls? If I read, aright, the signs of the times, I think we shall not always be contented with our inferior position. From looking down we have become almost blind, but may recover. In a new country, the necessities of life must be attended to first. The curse of Adam is upon us all, and we must earn our bread.

But it is the mission of applied science to render this easier for the whole world. There is a story which I once read that will illustrate the true position of applied science in the world. A boy, more fond of reading than of work, was employed, in the early days of the steam engine, to turn the valve at every stroke. Necessity was the mother of invention in his case: his reading was disturbed by his work, and he soon discovered that he might become free from his work by so tying the valve to some movable portion of the engine, as to make it move its own valve. So I consider that the true pursuit of mankind is intellectual. The scientific study of nature in all its branches, of mathematics, of mankind in its past and present, the pursuit of art, and the cultivation of all that is great and noble in the world—these are the highest occupation of mankind. Commerce, the applications of science, the accumulation of wealth, are necessities which are a curse to those with high ideals, but a blessing to that portion of the world which has neither the ability nor the taste for higher pursuits.

As the applications of science multiply, living becomes easier, the wealth necessary for the purchase of apparatus can better be obtained, and the pursuit of other things beside the necessities of life becomes possible.

But the moral qualities must also be cultivated in proportion to the wealth of the country, before much can be done in pure science. The successful sculptor or painter naturally attains to wealth through the legitimate work of his profession. The novelist, the poet, the musician, all have wealth before them as the end of a successful career. But the scientist and the mathematician have no such incentive to work, they must earn their living by other pursuits, usually teaching, and only devote their surplus time to the true pursuit of their science. And frequently, by the small salary which they receive, by the lack of instrumental and literary facilities, by the mental atmosphere in which they exist, and, most of all, by their low ideals of life, they are led to devote their surplus time to applied science or to other means of increasing their fortune. How shall we, then, honor the few, the very few, who, in spite of all difficulties, have kept their eyes fixed on the goal, and have steadily worked for pure science, giving to the world a most precious donation, which has borne fruit in our greater knowledge of the universe and in the applications to our physical life which have

enriched thousands and benefited each one of us? There are also those who have every facility for the pursuit of science, who have an ample salary and every appliance for work, yet who devote themselves to commercial work, to testifying in courts of law, and to any other work to increase their present large income. Such men would be respectable if they gave up the name of professor, and took that of consulting chemists or physicists. And such men are needed in the community. But for a man to occupy the professor's chair in a prominent college, and, by his energy and ability in the commercial applications of his science, stand before the local community in a prominent manner, and become the newspaper exponent of his science, is a disgrace both to him and his college. It is the death-blow to science in that region. Call him by his proper name, and he becomes at once a useful member of the community. Put in his place a man who shall by precept and example cultivate his science, and how different is the result! Young men, looking forward into the world for something to do, see before them this high and noble life, and they see that there is something more honorable than the accumulation of wealth. They are thus led to devote their lives to similar pursuits, and they honor the professor who has drawn them to something higher than they might otherwise have aspired to reach.

I do not wish to be misunderstood in this matter. It is no disgrace to make money by an invention, or otherwise, or to do commercial scientific work under some circumstances. But let pure science be the aim of those in the chairs of professors, and so prominently the aim that there can be no mistake. If our aim in life is wealth, let us honestly engage in commercial pursuits, and compete with others for its possession. But if we choose a life which we consider higher, let us live up to it, taking wealth or poverty as it may chance to come to us, but letting neither turn us aside from our pursuit.

The work of teaching may absorb the energies of many; and, indeed, this is the excuse given by most for not doing any scientific work. But there is an old saying, that where there is a will there is a way. Few professors do as much teaching or lecturing as the German professors, who are also noted for their elaborate papers in the scientific journals. I myself have been burdened down with work, and know what it is; and yet I here assert that all *can* find time for scientific research if they desire it. But here, again, that curse of our country, mediocrity, is upon us. Our colleges and universities seldom call for first-class men of reputation, and I have even heard the trustee of a well-known college assert that no professor should engage in research because of the time wasted! I was glad to see, soon after, by the call of a prominent scientist to that college, that the majority of the trustees did not agree with him.

That teaching is important, goes without saying. A successful teacher is to be respected; but if he does not lead his scholars to that which is highest, is he not blameworthy? We are, then, to look to the colleges and universities of the land for most of the work in pure science which is done. Let us, therefore, examine these latter and see what the prospect is.

One, whom perhaps we may here style a practical follower of Ruskin, has stated that while in this country he was variously designated by the title of captain, colonel and professor. The story may or may not be true, but we all know enough of the customs of our countrymen not to dispute it on general principles. All men are born equal: some men are captains, colonels and professors, and, therefore, all men are such. The logic is conclusive; and the same kind of logic seems to have been applied to our schools, colleges and universities. I have before me the report of the Commissioner of Education for 1880. According to that report, there were 389,* or say, in round numbers, 400 institutions, calling themselves colleges or universities, in our country! We may well exclaim that ours is a great country, having more than the whole world beside. The fact is sufficient. The whole earth would hardly support such a number of first-class institutions. The curse of mediocrity must be upon them, to swarm in such numbers. They must be a cloud of mosquitoes, instead of eagles as they profess. And this becomes evident on further analysis. About one-third aspire to the name of university; and I note one called by that name which has two professors and eighteen students, and another having three teachers and twelve students! And these instances are not unique, for the number of small institutions and schools which call themselves universities is very great. It is difficult to decide from the statistics alone the exact standing of these institutions. The extremes are easy to manage. Who can doubt that an institution with over 800 students, and a faculty of seventy, is of a higher grade than those above cited having ten or twenty students and two or three in the faculty? Yet this is not always true; for I note one institution with over 500 students which is known to me personally as of the grade of a high school. The statistics are more or less defective, and it would much weaken the force of my remarks if I went too much into detail. I append the following tables, however, of 330 so-called colleges and universities:

218	had from	0	to	100	students.
88	" "	100	"	200	"
12	" "	200	"	300	"
6	" "	300	"	500	"
6	over	500			

* Three hundred and sixty-four reported on, and twenty-five not reported.

Of 322 so-called colleges and universities:

206	had	0 to 10	in the	faculty.
99	"	10 " 20	"	"
17	"	20 or over	"	"

If the statistics were forthcoming—and possibly they may exist—we might also get an idea of the standing of these institutions and their approach to the true university idea, by the average age of the scholars. Possibly also the ratio of number of scholars to teachers might be of some help. All these methods give an approximation to the present standing of the institutions. But there is another method of attacking the problem, which is very exact, but it only gives us the *possibilities* of which the institution is capable. I refer to the wealth of the institution. In estimating the wealth, I have not included the value of grounds and buildings, for this is of little importance, either to the present or future standing of the institution. As good work can be done in a hovel as in a palace. I have taken the productive funds of the institution as the basis of estimate. I find:

234	have	below	$500,000
8	"	between	$500,000 and $1,000,000
8	"	over	$1,000,000

There is no fact more firmly established, all over the world, than that the higher education can never be made to pay for itself. Usually the cost to a college, of educating a young man, very much exceeds what he pays for it, and is often three or four times as much. The higher the education, the greater this proportion will be; and a university of the highest class should anticipate only a small accession to its income from the fees of students. Hence the test I have applied must give a true representation of the possibilities in every case. According to the figures, only sixteen colleges and universities have $500,000 or over of invested funds, and only one-half of these have $1,000,000 and over. Now, even the latter sum is a very small endowment for a college; and to call any institution a university which has less than $1,000,000, is to render it absurd in the face of the world. And yet more than 100 of our institutions, many of them very respectable colleges, have abused the word 'university' in this manner. It is to be hoped that the endowment of the more respectable of these institutions may be increased, as many of them deserve it; and their unfortunate appellation has probably been repented of long since.

But what shall we think of a community that gives the charter of a university to an institution with a total of $20,000 endowment, two so-called professors, and eighteen students! or another with three professors, twelve students, and a total of $27,000 endowment, mostly

invested in buildings! And yet there are very many similar institutions; there being sixteen with three professors or less, and very many indeed with only four or five.

Such facts as these could only exist in a democratic country, where pride is taken in reducing everything to a level. And I may also say that it can only exist in the early days of such a democracy; for an intelligent public will soon perceive that calling a thing by a wrong name does not change its character, and that truth, above all things, should be taught to the youth of the nation.

It may be urged that all these institutions are doing good work in education; and that many young men are thus taught who could not afford to go to a true college or university. But I do not object to the education—though I have no doubt an investigation would disclose equal absurdities here—for it is aside from my object. But I do object to lowering the ideals of the youth of the country. Let them know that they are attending a school, and not a university; and let them know that above them comes the college, and above that the university. Let them be taught that they are only half-educated, and that there are persons in the world by whose side they are but atoms. In other words, let them be taught the truth.

It may be that some small institutions are of high grade, especially those which are new; but who can doubt that more than two-thirds of our institutions calling themselves colleges and universities are unworthy of the name? Each one of these institutions has so-called professors, but it is evident that they can be only of the grade of teachers. Why should they not be so called? The position of teacher is an honored one, but is not made more honorable by the assumption of a false title. Furthermore, the multiplication of the title, and the ease with which it can be obtained, render it scarcely worth striving for. When the man of energy, ability and perhaps genius is rewarded by the same title and emoluments as the commonplace man with the modicum of knowledge, who takes to teaching, not because of any aptitude for his work, but possibly because he has not the energy to compete with his fellow-men in business, then I say one of the inducements for first-class men to become professors is gone.

When work and ability are required for the position, and when the professor is expected to keep up with the progress of his subject, and to do all in his power to advance it, and when he is selected for these reasons, then the position will be worth working for, and the successful competitor will be honored accordingly. The chivalric spirit which prompted Faraday to devote his life to the study of nature may actuate a few noble men to give their life to scientific work; but, if we wish to cultivate this highest class of men in science, we must open a career for them worthy of their efforts.

Jenny Lind, with her beautiful voice, would have cultivated it to some extent in her native village; yet who would expect her to travel over the world, and give concerts for nothing? and how would she have been able to do so if she had wished? And so the scientific man, whatever his natural talents, must have instruments and a library, and a suitable and respectable salary to live upon, before he is able to exert himself to his full capacity. This is true of advance in all the higher departments of human learning, and yet something more is necessary. It is not those in this country who receive the largest salary, and have positions in the richest colleges, who have advanced their subject the most: men receiving the highest salaries, and occupying the professor's chair, are to-day doing absolutely nothing in pure science, but are striving by the commercial applications of their science to increase their already large salary. Such pursuits, as I have said before, are honorable in their proper place; but the duty of a professor is to advance his science, and to set an example of pure and true devotion to it which shall demonstrate to his students and the world that there is something high and noble worth living for. Money-changers are often respectable men, and yet they were once severely rebuked for carrying on their trade in the court of the temple.

Wealth does not constitute a university, buildings do not: it is the men who constitute its faculty, and the students who learn from them. It is the last and highest step which the mere student takes. He goes forth into the world, and the height to which he rises has been influenced by the ideals which he has consciously or unconsciously imbibed in his university. If the professors under whom he has studied have been high in their profession, and have themselves had high ideals; if they have considered the advance of their particular subject their highest work in life, and are themselves honored for their intellect throughout the world—the student is drawn toward that which is highest, and ever after in life has high ideals. But if the student is taught by what are sometimes called good teachers, and teachers only, who know little more than the student, and who are often surpassed and even despised by him, no one can doubt the lowered tone of his mind. He finds that by his feeble efforts he can surpass one to whom a university has given its highest honor and he begins to think that he himself is a born genius, and the incentive to work is gone. He is great by the side of the molehill, and does not know any mountain to compare himself with.

A university should not only have great men in its faculty, but have numerous minor professors and assistants of all kinds, and should encourage the highest work, if for no other reason than to encourage the student to his highest efforts.

But, assuming that the professor has high ideals, wealth such as

only a large and high university can command, is necessary to allow him the fullest development.

And this is specially so in our science of physics. In the early days of physics and chemistry, many of the fundamental experiments could be performed with the simplest apparatus. And so we often find the names of Wollaston and Faraday mentioned as needing scarcely anything for their researches. Much can even now be done with the simplest apparatus; and nobody, except the utterly incompetent, need stop for want of it. But the fact remains that one can only be free to investigate in all departments of chemistry and physics, when he not only has a complete laboratory at his command, but a friend to draw on for the expenses of each experiment. That simplest of the departments of physics, namely, astronomy, has now reached such perfection that nobody can expect to do much more in it without a perfectly equipped observatory; and even this would be useless without an income sufficient to employ a corps of assistants to make the observations and computations. But even in this simplest of physical subjects, there is great misunderstanding. Our country has very many excellent observatories: and yet little work is done in comparison, because no provision has been made for maintaining the work of the observatory; and the wealth which, if concentrated, might have made one effective observatory which would prove a benefit to astronomical science, when scattered among a half-dozen, merely furnishes telescopes for the people in the surrounding region to view the moon with. And here I strike the keynote of at least one need of our country, if she would stand well in science; and the following item which I clip from a newspaper will illustrate the matter:

"The eccentric old Canadian, Arunah Huntington, who left $200,000 to be divided among the public schools of Vermont, has done something which will be of little practical value to the schools. Each district will be entitled to the insignificant sum of $10, which will not advance much the cause of education."

Nobody will dispute the folly of such a bequest, or the folly of filling the country with telescopes to look at the moon, and calling them observatories. How much better to concentrate the wealth into a few parcels, and make first-class observatories and institutions with it!

Is it possible that any of our four hundred colleges and universities have love enough of learning to unite with each other and form larger institutions? Is it possible that any have such a love of truth that they are willing to be called by their right name? I fear not; for the spirit of expectation, which is analogous to the spirit of gambling, is strong in the American breast, and each institution which now, except in name, slumbers in obscurity, expects in time to bloom out into full prosperity. Although many of them are under religious influence,

where truth is inculcated, and where men are taught to take a low seat at the table in order that they may be honored by being called up higher, and not dishonored by being thrust down lower, yet these institutions have thrust themselves into the highest seats, and cannot probably be dislodged.

But would it not be possible to so change public opinion that no college could be founded with a less endowment than say $1,000,000, or no university with less than three or four times that amount? From the report of the Commissioner of Education I learn that such a change is taking place; that the tendency towards large institutions is increasing, and that it is principally in the West and Southwest that the multiplication of small institutions with big names is to be feared most, and that the East is almost ready for the great coming university.

The total wealth of the four hundred colleges and universities in 1880 was about $40,000,000 in buildings and $43,000,000 in productive funds. This would be sufficient for one great university of $10,000,000, four of $5,000,000, and twenty-six colleges of $2,000,000 each. But such an idea can of course never be carried out. Government appropriations are out of the question, because no political trickery must be allowed around the ideal institution.

In the year 1880 the private bequests to all schools and colleges amounted to about $5,500,000; and, although there was one bequest of $1,250,000, yet the amount does not appear to be phenomenal. It would thus seem that the total amount was about five million dollars in one year, of which more than half is given to so-called colleges and universities. It would be very difficult to regulate these bequests so that they might be concentrated sufficiently to produce an immediate result. But the figures show that generosity is a prominent feature of the American people, and that the needs of the country only have to be appreciated to have the funds forthcoming. We must make the need of research and of pure science felt in the country. We must live such lives of pure devotion to our science, that all shall see that we ask for money, not that we may live in indolent ease at the expense of charity, but that we may work for that which has advanced and will advance the world more than any other subject, both intellectually and physically. We must live such lives as to neutralize the influence of those who in high places have degraded their profession, or have given themselves over to ease, and do nothing for the science which they represent. Let us do what we can with the present means at our disposal. There is not one of us who is situated in the position best adapted to bring out all his powers, and to allow him to do most for his science. All have their difficulties, and I do not think that circumstances will ever radically change a man. If a man has the instinct of research in him, it will always show itself in some form.

But circumstances may direct it into new paths, or may foster it so that what would otherwise have died as a bud now blossoms and ripens into the perfect fruit.

Americans have shown no lack of invention in small things; and the same spirit, when united to knowledge and love of science, becomes the spirit of research. The telegraph-operator, with his limited knowledge of electricity and its laws, naturally turns his attention to the improvement of the only electrical instrument he knows anything about; and his researches would be confined to the limited sphere of his knowledge, and to the simple laws with which he is acquainted. But as his knowledge increases, and the field broadens before him, as he studies the mathematical theory of the subject, and the electro-magnetic theory of light loses the dim haze due to distance, and becomes his constant companion, the telegraph-instrument becomes to him a toy, and his effort to discover something new becomes research in pure science.

It is useless to attempt to advance science until one has mastered the science: he must step to the front before his blows can tell in the strife. Furthermore, I do not believe anybody can be thorough in any department of science, without wishing to advance it. In the study of what is known, in the reading of the scientific journals, and the discussions therein contained of the current scientific questions, one would obtain an impulse to work, even though it did not before exist. And the same spirit which prompted him to seek what was already known, would make him wish to know the unknown. And I may say that I never met a case of thorough knowledge in my own science, except in the case of well-known investigators. I have met men who talked well, and I have sometimes asked myself why they did not do something; but further knowledge of their character has shown me the superficiality of their knowledge. I am no longer a believer in men who could do something if they would, or would do something if they had a chance. They are impostors. If the true spirit is there, it will show itself in spite of circumstances.

As I remarked before, the investigator in pure science is usually a professor. He must teach as well as investigate. It is a question which has been discussed in late years, as to whether these two functions would better be combined in the same individual, or separated. It seems to be the opinion of most that a certain amount of teaching is conducive, rather than otherwise, to the spirit of research. I myself think that this is true, and I should myself not like to give up my daily lecture. But one must not be overburdened. I suppose that the true solution, in many cases, would be found in the multiplication of assistants, not only for the work of teaching but of research. Some men are gifted with more ideas than they can work out with their own

hands, and the world is losing much by not supplying them with extra hands. Life is short: old age comes quickly, and the amount one pair of hands can do is very limited. What sort of shop would that be, or what sort of factory, where one man had to do all the work with his own hands? It is a fact in nature, which no democracy can change, that men are *not* equal—that some have brains and some hands. And no idle talk about equality can ever subvert the order of the universe.

I know of no institution in this country where assistants are supplied to aid directly in research. Yet why should it not be so? And even the absence of assistant professors and assistants of all kinds, to aid in teaching, is very noticeable, and must be remedied before we can expect much.

There are many physical problems, especially those requiring exact measurements, which cannot be carried out by one man, and can only be successfully attacked by the most elaborate apparatus, and with a full corps of assistants. Such are Regnault's experiments on the fundamental laws of gases and vapors, made thirty or forty years ago by aid from the French Government, and which are the standards to this day. Although these experiments were made with a view to the practical calculation of the steam engine, yet they were carried out in such a broad spirit that they have been of the greatest theoretical use. Again, what would astronomy have done without the endowments of observatories? By their means, that science has become the most perfect of all branches of physics, as it should be from its simplicity. There is no doubt, in my mind, that similar institutions for other branches of physics, or, better, to include the whole of physics, would be equally successful. A large and perfectly equipped physical laboratory with its large revenues, its corps of professors and assistants, and its machine shop for the construction of new apparatus, would be able to advance our science quite as much as endowed observatories have astronomy. But such a laboratory should not be founded rashly. The value will depend entirely on the physicist at its head, who has to devise the plan, and to start it into practical working. Such a man will always be rare, and cannot always be obtained. After one had been successfully started, others could follow; for imitation requires little brains.

One could not be certain of getting the proper man every time, but the means of appointment should be most carefully studied so as to secure a good average. There can be no doubt that the appointment should rest with a scientific body capable of judging the highest work of each candidate.

Should any popular element enter, the person chosen would be either of the literary-scientific order, or the dabbler on the outskirts who presents his small discoveries in the most theatrical manner. What

is required is a man of depth, who has such an insight into physical science that he can tell when blows will best tell for its advancement.

Such a grand laboratory as I describe does not exist in the world, at present, for the study of physics. But no trouble has ever been found in obtaining means to endow astronomical science. Everybody can appreciate, to some extent, the value of an observatory; as astronomy is the simplest of scientific subjects, and has very quickly reached a position where elaborate instruments and costly computations are necessary to further advance. The whole domain of physics is so wide that workers have hitherto found enough to do. But it cannot always be so, and the time has even now arrived when such a grand laboratory should be founded. Shall our country take the lead in this matter, or shall we wait for foreign countries to go before? They will be built in the future, but when and how is the question.

Several institutions are now putting up laboratories for physics. They are mostly for teaching, and we can expect only a comparatively small amount of work from most of them. But they show progress; and, if the progress be as quick in this direction as in others, we should be able to see a great change before the end of our lives.

As stated before, men are influenced by the sympathy of those with whom they come in contact. It is impossible to immediately change public opinion in our favor; and, indeed, we must always seek to lead it, and not be guided by it. For pure science is the pioneer that must not hover about cities and civilized countries, but must strike into unknown forests, and climb the hitherto inaccessible mountains which lead to and command a view of the promised land—the land that science promises us in the future; which shall not only flow with milk and honey, but shall give us a better and more glorious idea of this wonderful universe. We must create a public opinion in our favor, but it need not be at first the general public. We must be contented to stand aside, and see the honors of the world for a time given to our inferiors; and must be better contented with the approval of our own consciences, and of the very few who are capable of judging our work, than of the whole world beside. Let us look to the other physicists, not in our own town, not in our own country, but in the whole world, for the words of praise which are to encourage us, or the words of blame which are to stimulate us to renewed effort. For what to us is the praise of the ignorant? Let us join together in the bonds of our scientific societies, and encourage each other, as we are now doing, in the pursuit of our favorite study; knowing that the world will some time recognize our services, and knowing, also, that we constitute the most important element in human progress.

But danger is also near, even in our societies. When the average tone of the society is low, when the highest honors are given to the

mediocre, when third-class men are held up as examples, and when trifling inventions are magnified into scientific discoveries, then the influence of such societies is prejudicial. A young scientist attending the meetings of such a society soon gets perverted ideas. To his mind, a molehill is a mountain, and the mountain a molehill. The small inventor or the local celebrity rises to a greater height, in his mind, than the great leader of science in some foreign land. He gauges himself by the molehill, and is satisfied with his stature; not knowing that he is but an atom in comparison with the mountain, until, perhaps, in old age, when it is too late. But, if the size of the mountain had been seen at first, the young scientist would at least have been stimulated in his endeavor to grow.

We cannot all be men of genius; but we can, at least, point them out to those around us. We may not be able to benefit science much ourselves; but we can have high ideals on the subject, and instil them into those with whom we come in contact. For the good of ourselves, for the good of our country, for the good to the world, it is incumbent on us to form a true estimate of the worth and standing of persons and things, and to set before our own minds all that is great and good and noble, all that is most important for scientific advance, above the mean and low and unimportant.

It is very often said that a man has a right to his opinion. This might be true for a man on a desert island, whose error would influence only himself. But when he opens his lips to instruct others, or even when he signifies his opinions by his daily life, then he is directly responsible for all his errors of judgment or fact. He has no right to think a molehill as big as a mountain, nor to teach it, any more than he has to think the world flat, and teach that it is so. The facts and laws of our science have *not* equal importance, neither have the men who cultivate the science achieved equal results. One thing is greater than another, and we have no right to neglect the order. Thus shall our minds be guided aright, and our efforts be toward that which is the highest.

Then shall we see that no physicist of the first class has ever existed in this country, that we must look to other countries for our leaders in that subject, and that the few excellent workers in our country must receive many accessions from without before they can constitute an American science, or do their share in the world's work.

But let me return to the subject of scientific societies. Here American science has its hardest problem to contend with. There are very many local societies dignified by high-sounding names, each having its local celebrity, to whom the privilege of describing some crab with an extra claw, which he found in his morning ramble, is inestimable. And there are some academies of science, situated at our seats of learn-

ing, which are doing good work in their locality. But distances are so great that it is difficult to collect men together at any one point. The American Association, which we are now attending, is not a scientific academy, and does not profess to be more than a gathering of all who are interested in science, to read papers and enjoy social intercourse. The National Academy of Sciences contains eminent men from the whole country, but then it is only for the purpose of advising the government freely on scientific matters. It has no building, it has no library; and it publishes nothing except the information which it freely gives to the government, which does nothing for it in return. It has not had much effect directly on American science; but the liberality of the government in the way of scientific expeditions, publications, etc., is at least partly due to its influence, and in this way it has done much good. But it in no way takes the place of the great Royal Society, or the great academies of science at Paris, Berlin, Vienna, St. Petersburg, Munich, and, indeed, all the European capitals and large cities. These, by their publications, give to the young student, as well as the more advanced physicist, models of all that is considered excellent; and to become a member is one of the highest honors to which he can aspire, while to write a memoir which the academy considers worthy to be published in its transactions excites each one to his highest effort.

The American Academy of Sciences in Boston is perhaps our nearest representation of this class of academies, but its limitation of membership to the State deprives it of its national character.

But there is another matter which influences the growth of our science.

As it is necessary for us still to look abroad for our highest inspiration in pure science, and as science is not an affair of one town or one country, but of the whole world, it becomes us all to read the current journals of science and the great transactions of foreign societies, as well as those of our own countries. These great transactions and journals should be in the library of every institution of learning in the country, where science is taught. How can teachers and professors be expected to know what has been discovered in the past, or is being discovered now, if these are not provided? Has any institution a right to mentally starve the teachers whom it employs, or the students who come to it? There can be but one answer to this; and an institution calling itself a university, and not having the current scientific journals upon its table or the transactions of societies upon its library shelves, is certainly not doing its best to cultivate all that is best in this world.

We call this a free country, and yet it is the only one where there is a direct tax upon the pursuit of science. The low state of pure

science in our country may possibly be attributed to the youth of the country; but a direct tax, to prevent the growth of our country in that subject, cannot be looked upon as other than a deep disgrace. I refer to the duty upon foreign books and periodicals. In our science, no books above elementary ones have ever been published, or are likely to be published, in this country; and yet every teacher in physics must have them, not only in the college library, but on his own shelves, and must pay the government of this country to allow him to use a portion of his small salary to buy that which is to do good to the whole country. All freedom of intercourse which is necessary to foster our growing science is thus broken off; and that which might, in time, relieve our country of its mediocrity, is nipped in the bud by our government, which is most liberal when appealed to directly on scientific subjects.

One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off. Our scientific associations and societies must make themselves heard in this matter, and show those in authority how the matter stands.

In conclusion, let me say once more, that I do not believe that our country is to remain long in its present position. The science of physics, in whose applications our country glories, is to arise among us, and make us respected by the nations of the world. Such a prophecy may seem rash with regard to a nation which does not yet do enough physical work to support a physical journal. But we do know the speed with which we advance in this country: we see cities springing up in a night, and other wonders performed at an unprecedented rate. And now we see physical laboratories being built, we see a great demand for thoroughly trained physicists, who have not shirked their mathematics, both as professors and in so-called practical life; and perhaps we have the feeling, common to all true Americans, that our country is going forward to a glorious future, when we shall lead the world in the strife for intellectual prizes as we now do in the strife for wealth.

But if this is to be so, we must not aim low. The problems of the universe cannot be solved without labor: they cannot be attacked without the proper intellectual as well as physical tools; and no physicist need expect to go far without his mathematics. No one expects a horse to win in a great and long race who has not been properly trained; and it would be folly to attempt to win with one, however pure his blood and high his pedigree, without it. The problems we solve are more difficult than any race: the highest intellect cannot hope to succeed without proper preparation. The great prizes are reserved for the

greatest efforts of the greatest intellects, that have kept their mental eye bright and flesh hard by constant exercise. Apparatus can be bought with money, talents may come to us at birth; but our mental tools, our mathematics, our experimental ability, our knowledge of what others have done before us, all have to be obtained by work. The time is almost past, even in our own country, when third-rate men can find a place as teachers, because they are unfit for everything else. We wish to see brains and learning, combined with energy and immense working-power, in the professor's chair; but, above all, we wish to see that high and chivalrous spirit which causes one to pursue his idea in spite of all difficulties, to work at the problems of nature with the approval of his own conscience, and not of men before him. Let him fit himself for the struggle with all the weapons which mathematics and the experience of those gone before him can furnish, and let him enter the arena with the fixed and stern purpose to conquer. Let him not be contented to stand back with the crowd of mediocrity, but let him press forward for a front place in the strife.

The whole universe is before us to study. The greatest labor of the greatest minds has given us only a few pearls; and yet the limitless ocean, with its hidden depths filled with diamonds and precious stones, is before us. The problem of the universe is yet unsolved, and the mystery involved in one single atom yet eludes us. The field of research only opens wider and wider as we advance, and our minds are lost in wonder and astonishment at the grandeur and beauty unfolded before us. Shall we help in this grand work, or not? Shall our country do its share, or shall it still live in the almshouse of the world?

THE MALARIA-GERM AND ALLIED FORMS OF SPOROZOA.

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THE group of animal parasites to which the malaria-causing organism belongs is relatively unimportant when compared with the bacteria, a group of plant parasites, including the causes of most zymotic diseases in man—typhoid, cholera, diphtheria, scarlet fever, tuberculosis and the like, as well as beneficial forms which aid man in various ways. Nevertheless, it is a group of considerable economic importance, about which little is known outside of scientific circles. The name Sporozoa suggests, to the average reader, no disquieting apprehension of physical pain or of financial loss, yet this class of primitive, unicellular animals includes, besides the malaria-causing blood parasites, forms which, like the silkworm parasite *(Glugea bombycis)*, have cost communities untold millions of dollars. In connection with the losses due to one of these silkworm epidemics, Huxley writes in 1870:

"In the years following 1853 this malady broke out with such extreme violence that, in 1853, the silk crop was reduced to a third of the amount which it had reached in 1853; and up till within the last year or two it has never attained half the yield of 1853. This means not only that the great number of people engaged in silk growing are some thirty millions sterling poorer than they might have been; it means not only that high prices have had to be paid for imported silkworm eggs, and that, after investing his money in them, in paying for mulberry leaves and for attendance, the cultivator has constantly seen his silkworms perish and himself plunged in ruin; but it means that the looms of Lyons have lacked employment, and that, for years, enforced idleness and misery have been the portion of a vast population which, in former days, was industrious and well to do."

Analogous epidemics, which may be traced to Sporozoa, are liable to break out at any time among other animals having commercial value. Thus 'Texas fever,' a cattle disease due to a sporozoan blood parasite *(Piroplasma bigeminum)*, occasions great loss to cattle breeders. Muscle parasites, belonging to the same class, cause trichinosis-like diseases in hogs, cows, cats, dogs and other domestic animals; while in fish they occasion great loss to fish-culturists through epidemics. Other parasites in the same class are the causes of disease in horses, sheep, goats, etc.

The Sporozoa are comparatively harmless to man personally, but, unlike some bacteria, they are never beneficial in any sense. Invariably parasites, the diseases which they induce are confined mainly to the lower animals, but so widely are they distributed that no type of animals is free from them altogether. One significant feature about the

Sporozoa is that, notwithstanding the many kinds and the wide distribution in all sorts of hosts, the life history of the parasites invariably conforms to the same type, a fact which has recently been used to good advantage in working out the development of the malaria organism.

Like all the unicellular animals, or Protozoa, the Sporozoa are minute bits of protoplasm provided with a membrane and a specialized spherical portion of the inner protoplasm called the nucleus. Unlike the other Protozoa, they are entirely devoid of motile organs and are, in consequence, quiescent. In classifying them, advantage has been taken of the different modes in which they form spores, or germs, by which they are reproduced. In some, known as the *Telosporidia*, all the protoplasm of the parasite is used to form the spores, and the parent cell dies or disappears with each sporulation, which thus represents the end of the individual parasite. The individuals of the second group, known as the *Neosporidia*, form spores, without using all the protoplasm, and continue to live after each sporulation. This group comprises the less-known forms of Sporozoa, and is of considerable economic importance as the cause of epidemics among silkworms, brook trout and other fish, etc.

The Telosporidia are further divided according to the mode of life. Some of them, known as the *Gregarinida*, live in cavities of the body of many forms of invertebrates, but rarely in vertebrates; others, the *Coccidia*, live in epithelial cells lining the cavities of both invertebrate and vertebrate hosts. It may be remarked, parenthetically, that the cause of cancerous growths in man is claimed by many to be organisms belonging to this group of Sporozoa. The question remains in considerable doubt, however, and, despite the great mass of literature, no positive results have appeared. The last group finally of the Telosporidia is the *Hæmosporidia*, comprising parasites which, like the malaria-organism—*Plasmodium malariæ*—live in blood corpuscles of vertebrates.

All these different types of Telosporidia begin life as minute germs called *sporozoites*, which make their way into the new host through the intestine, being taken in with the food. The life history, after this ingestion, follows slightly modified paths in the different types, and, for purposes of comparison, I will describe these processes in the gregarine, the coccidium and in the hæmospore *Plasmodium malariæ*, thus representing each of the subdivisions of the Telosporidia.

The sea-squirt, or Tunicate, *Ciona intestinalis*, is the host of a gregarine *Monocystis ascidiæ*, which is so widely distributed that it is almost impossible to find a *Ciona* without them. The complete life history of the parasite has been fully worked out by Prof. M. Siedlecki,

of Cracow University, Russia, whose results form the basis of the following account*:

The description of the life cycle may begin with the sporozoite, or youngest form, of the gregarine parasite. This is a small, elongate germ which makes its way through the fluids of the digestive tract of *Ciona* to the epithelial cells which line that canal. (Fig. 1, A.) The sporozoite penetrates one of these cells and begins to grow at the expense of the cell contents, until, finally, too large for the cell host, it breaks the cell wall and falls into the lumen of the digestive tract, where it soon attains its full size. (Fig. 1, B. C. D.) It is now a comparatively large, sac-like cell, swollen at one end, and with a distinct nucleus. (Fig. 1, C.) After a longer or shorter period, not definitely determined, two adult forms come together and pour out a sticky, fluid substance, which soon hardens to form a common, firm covering, or cyst. (Fig. 1. E.) Each nucleus then begins to divide, and, after a multitude of daughter-nuclei have arisen, the protoplasm of the cell breaks up into as many parts as there are nuclei (Fig. 1, F. G. H.). These small protoplasmic parts *(gametes)* then wander out of the parent membranes and ultimately fuse, two by two, while still remaining in the original cyst wall (Fig. 1. I. J.). After the fusion, the nucleus and protoplasm in each double mass divides into eight parts, and a firm, enveloping membrane is secreted about them. This spore-membrane ultimately becomes impregnated with calcareous material, which thus forms a firm and resisting capsule for the eight germs within. Each germ is a sporozoite similar to the one which began the life cycle.

During the process of sporozoite-formation, the parasite is passed out with the fæces to the exterior. Here the original cyst ultimately bursts and liberates the multitude of spores with their contained sporozoites. The latter are well protected, however, by their calcareous shells, and do not suffer from the sea water or from drying. The spores may be finally taken into the digestive tract with food, and with this the opportunity for a renewed cycle is presented. The acids of the digestive fluids dissolve the calcareous coverings, and the eight sporozoites in each spore are liberated. The sporozoites again penetrate the epithelial cells, grow to maturity and repeat the process indefinitely.

In *Coccidium*, a parasite of some of the insects, the life history as worked out by Dr. F. Schaudinn differs in one or two important points from that of the gregarine.

Sporozoites are formed as in the previous case, and these work their way in a similar manner into the cells lining the digestive tract (Fig 2, *a*). Unlike the gregarine, the main period of their life is passed in these cells, and they drop into the lumen of the intestine only when they

* Siedlicki. Ueber die geschlechliche Vermehrung der Monocystis ascidiæ R. Lank. Bull. d. l'Acad. d. Sci. d. Cracovie, December, 1899.

are ready to form spores. The nucleus divides repeatedly, and a great number of buds are formed around the daughter nuclei (Fig. 2, *b, c*). These buds elongate from the periphery of the parent organism and radiate from it, like the spines of a sea urchin. When fully developed, the spores, or, as they are technically known, the *merozoites,* drop off the parent cell and work their way through the fluids of the digestive tract until they come to the cells lining it, and then, like the sporozoites, they penetrate the cells, grow at their expense, and again reproduce spores as before (Fig 2, *a* to *c*). This process thus tends to spread the disease among the cells of the digestive tract in the one host, and it will be observed that the reproductive process is not accompanied by the union of two gametes, as in the case of *Monocystis*. *Coccidium* is thus distinguished from the latter in having a method of asexual multiplication leading to auto-infection. This process, however, cannot continue indefinitely, and, after five or six days, a method of sexual multiplication supervenes. The preliminary stages of this process do not differ from the formation of the merozoites, and similar buds are formed which break off and penetrate the epithelial cells as before. The further history, however, differs markedly from that of the merozoite. Some of the resulting parasites give rise to immense numbers of minute, active, thread-like buds, the *microgametes,* which radiate from the parent cell like the merozoites *(h—j)*. Others do not form buds at all, but merely enlarge until they are as large, or larger than, the ordinary full-grown parasites *(d—f)*. One of the small forms then fuses with a large form, in conjunction; and the result, or *copula,* secretes a firm cyst about itself, and then divides into spores (2, *g*). Each spore then secretes about itself a second coating which becomes impregnated with calcareous matter, and, within this cyst, the cell divides into a small number of sporozoites *(k)*. In this condition the primary cysts are emptied to the outside, where they are ultimately taken up by some new host in whose digestive tract the cysts are dissolved and the sporozoites liberated to renew the cycle (Fig. 2, *l*).

It thus appears that, in *Coccidium,* the life cycle is more complicated than in the gregarine, in having a period of asexual reproduction by which auto-infection is accomplished, alternating with a period of sexual multiplication during which the parasite is carried from one host to another. *Coccidium* differs further from *Monocystis* in that the conjugating gametes are sexually differentiated, the small, active one, or microgamete, functions as the male cell, and the larger, quiescent one, or macrogamete, as the female or egg cell, while in the gregarine, on the other hand, the conjugating gametes are of equal size.

We may now consider the somewhat more complicated life cycle of the malaria organism. The process of spore-formation of this para-

site, in the blood cells of the human host, was correctly made out in 1888 by two Italian naturalists, Marchiafava and Celli, who showed that the young parasite, in a red blood corpuscle, is a minute granule in which no structure could be made out. The granule grows, however, at the expense of the hæmaglobin of the corpuscle, and ultimately forms spores (Fig. 3, *a—f*). During the life of the parent organism, the products of growth are stored up in the parasite in the form of fine gran-

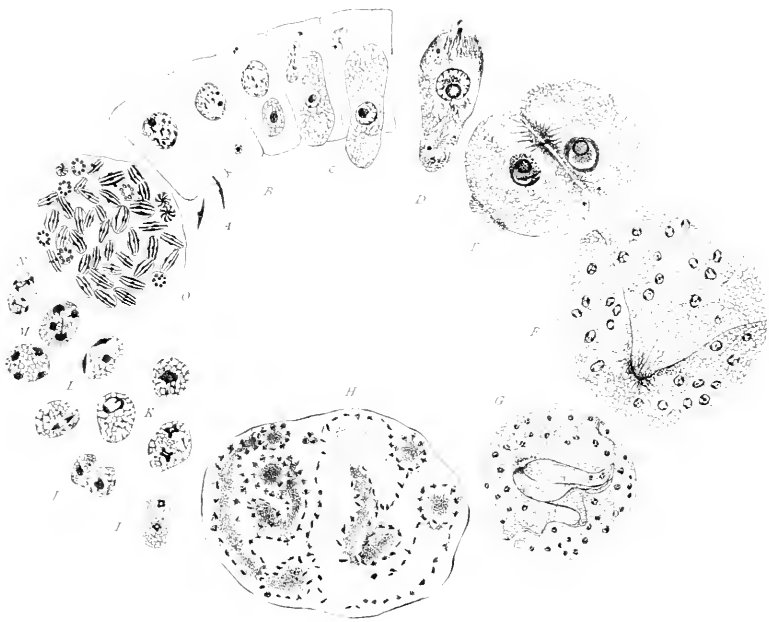

FIG. 1. LIFE CYCLE OF A GREGARINE. [MAINLY AFTER SIEDLECKI.]

Minute germs, sporozoites (A), enter epithelial cells lining the digestive tract of a tunicate. Here they grow to a large size (B, C), ultimately breaking through a cell-membrane and falling into the lumen of the digestive tract (D). After some time in this adult condition, two individuals come together (E). The nuclei divide repeatedly (F, G), and minute gametes are ultimately formed (H, I). The gametes then fuse, two by two (I, J), forming the spores. The two nuclei also fuse (K), and the joint nucleus then divides three times in succession (L, M, N), forming right daughter-nuclei, which become the nuclei of eight germs or sporozoites (O). The sporozoites are inclosed in small calcareous capsules which, in a new host, are dissolved by the acids of the digestive fluids, thus setting free the sporozoites (A).

ules. These, known as *melanin* granules, are left in the center of the parent organism when the spores are formed, but at this period the blood corpuscle, in which the sporulation occurs, disintegrates, and so liberates the spores and the melanin in the blood plasm. Like the merozoites of *Coccidia*, these spores make their way to new corpuscles, which they

enter, and in which they repeat the cycle, thus bringing about auto-infection.

Another Italian naturalist, Golgi, in 1889, showed that the spore-formation of the parasites and the well-known pyrexial attacks on the part of the patient occur at the same time, and the phenomena were interpreted as cause and effect. The direct cause of the attack was then found to be the liberation into the blood plasm of the melanin

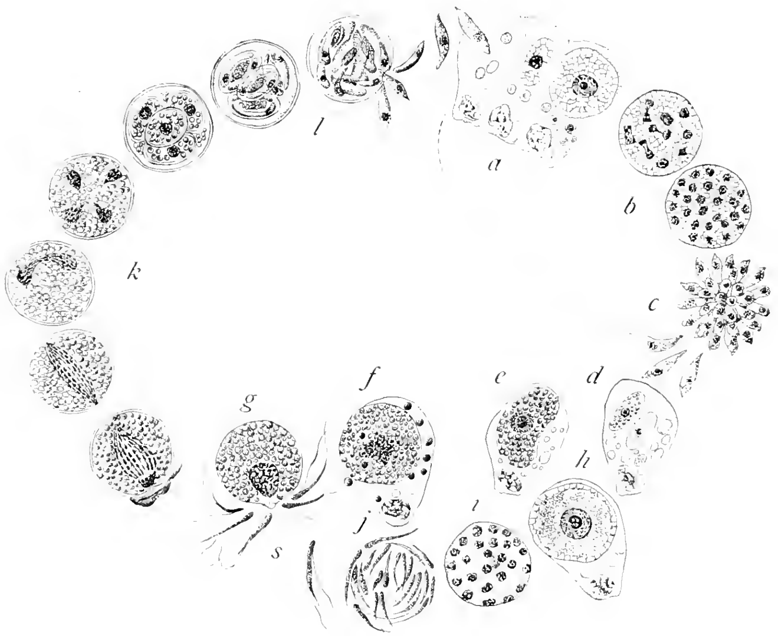

FIG. 2. LIFE CYCLE OF COCCIDIUM. [SCHAUDINN.]

The sporozoites penetrate epithelial cells (*a*) and grow to adult size. When ready to sporulate, they are free in the lumen of the organ. The nucleus divides repeatedly (*b*) and each of the ultimate sub-divisions becomes the nucleus of a merozoite (*c*). These re-enter epithelial cells (*a*) and repeat the cycle. After five or six days the merozoites have a different fate. Some (*d*—*g*) enlarge and form egg-cells; others (*h*—*j*) form minute flagellated male cells, or microgametes. One of these fuses with one egg cell, (*g*—*k*), and the copula then forms spores (*k*), each of which form, in turn, two sporozoites (*l*). In this condition the organism is taken into a new host and the process is then repeated.

granules, which, acting like a poison, throw the entire system into disorder. In different types of malaria, the attacks sometimes occur every 72 hours, sometimes every 48 hours, and in some cases at irregular intervals. These different effects are produced by slightly different forms of the malaria organism. One form, known as *Plasmodium malariæ*, sporulates every 72 hours; another, *Plasmodium*

vivax, every 48 hours. Another form, giving rise to a more malignant type of malaria, is *Laverania malariæ*, which probably sporulates every 48 hours. In some types of the disease it is supposed that two or more

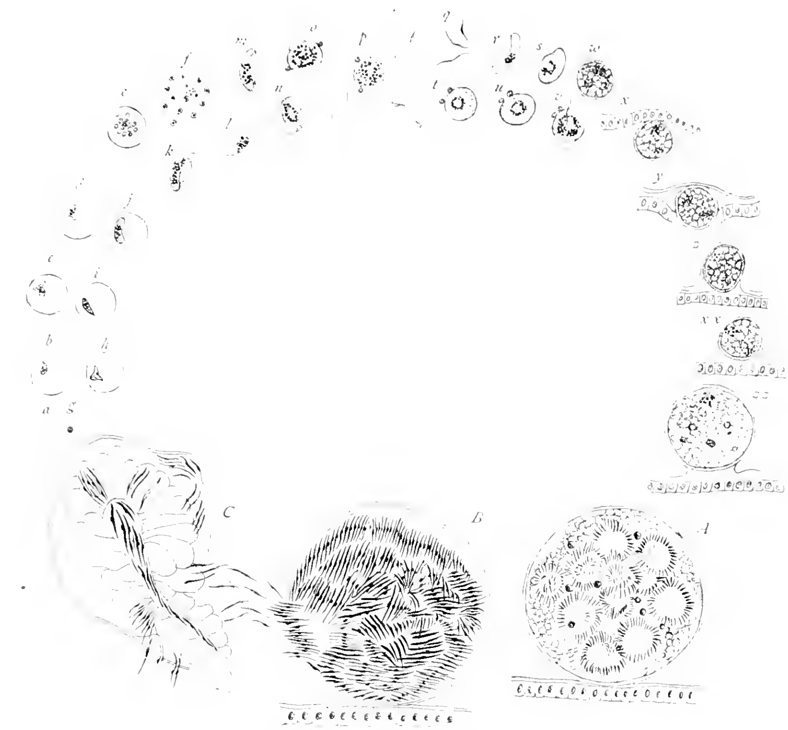

Fig. 3. Life Cycle of the Malaria-Organism. [Partly after Ross and Fielding-Ould.]

The young sporozoites (*a*) penetrate red blood corpuscles and grow at the expense of the hæmaglobin (*b*—*d*). Spores (*e*—*f*) are finally formed and the growth products (melanin) are liberated. These spores, or merozoites, enter new blood cells and repeat the process (auto-infection). When taken into the stomach of the mosquito *Anopheles*, the parasites become much larger, some develop male gametes (*o*, *p*, *q*) on the *Polymitus* form (*p*); others, female gametes (*r*—*u*). These conjugate (*v*) in the stomach of *Anopheles*, and the copula (*w*) penetrates the epithelial lining of the stomach and ultimately lies suspended in the body cavity (*x*—*zz*). When mature, the parasite forms spores (A), which form, in turn, naked sporozoites (B). The sporozoites are liberated into the body cavity, and finally penetrate the salivary gland of the mosquito (C), from which they are emptied out with the fluid from the gland into the blood of a human host.

species may be present at the same time, and, sporulating at different intervals, may give rise to irregular attacks.

Until 1896, the life history of the parasite, as outlined above, was regarded as incomplete because of the perplexing form discovered in

the blood of malaria patients by Professor Danilewsky, in 1891. This form differed from the ordinary parasites in having many fine, flagelliform appendages, which, breaking away from the parent, would swim about freely in the surrounding fluid (Fig. 3, *p*). Danilewsky regarded this form as an independent blood parasite, and gave to it the name *Polymitus*. In a sense, *Polymitus* has been the key to the life history of the malaria organism, and its history has been the history of the further discoveries upon malaria. In France, Laveran regarded Danilewsky's discovery as indicating some stage in the cycle of *Plasmodium malariæ*, and not as an independent organism, while Labbé considered *Polymitus* a degenerate condition of the ordinary parasites and without further significance. The English specialist on tropical diseases, Dr. Manson, found that the malaria parasites, when exposed with the blood to the cooler air, very soon assume the *Polymitus* form, which he regarded as the extra-corporeal form assumed by the malaria-organism, for, he argued, the wide distribution of malaria, the spread from individual to individual, can be explained, since the disease is not contagious, only by the assumption of germs outside of the body. Furthermore, he suggested that these germs might be carried from person to person, by insects, such as the mosquito. In the same year, Laveran made an identical suggestion quite independently of Manson. It was not altogether novel, however, with either of these investigators; thus a certain mosquito in central Africa is known to the natives as the fever organism, while the same idea was represented in Theobald Smith's discovery (1893) of the tick as the agent in the transmission of the 'Texas fever' of cattle.

The first positive results on the significance of the *Polymitus* form were obtained by MacCallum, in Washington, in 1897-'98. A similar *Polymitus* form is developed by the malaria-organism of birds *(Halteridium)*, and, in the blood of diseased crows, MacCallum observed the filamentous motile bodies of the *Polymitus* form, break away from the central mass, and unite with an ordinary parasite. The result of the conjugation was a copula with an independent motion, by which it made its way through the surrounding fluids. The later history of the copula was not followed; similar observations were made, by the same observer, upon living malaria-organisms of man, and the *Polymitus* 'flagella' were seen to unite with larger forms of the parasite.

In the meantime, Major Ross, in India, was working out the mosquito hypothesis of Manson and Laveran, and succeeded in placing that theory upon a very substantial basis. He found black pigment granules, in the intestine and epithelial cells of the mosquito, which were identified as melanin granules of the blood parasite. It was also found, by Ross, that only certain kinds of mosquitoes were selected by the malaria parasites, *viz.:* various species of the genus *Anopheles*

by the human parasite, while species of the genus *Culex* were selected by the malaria parasites of birds. The full history, finally, has been worked out in complete detail during the last two years, by Ross again, and by Grassi, in Italy, and both observers reached quite independent, but identical, results. Briefly summarizing these results, the full life history of *Plasmodium malariæ* may be given as follows:

The early form of the parasite, which corresponds with the sporozoites of the gregarine and of *Coccidium*, penetrates a red blood corpuscle, grows to adult size, and then forms spores (Fig. 3, *a—b*). These correspond exactly with the merozoites of the *Coccidia*, and, like them, lead to auto-infection. At this point there is a gap in the evidence, for it is not known how long this asexual method of increase may continue; as shown above in the case of *Coccidium*, the spore-forming period continues for five or six days, when a period of conjugation supervenes. It may be stated here, parenthetically, that in all Protozoa, so far as known, a period of conjugation is necessary at some time during the life cycle, and without such conjugation, the organisms, which are reproduced asexually, finally decrease in size and show other signs of degeneration, ultimately resulting in death of the race (see results of Bütschli, Maupas, Hertwig, etc., upon degenerating Protozoa).

This is of considerable moment in the question of malaria, for, if the malaria-organism conforms to other Protozoa, there must come a time when this asexual sporulation will cease in any given set of individuals, and a period of conjugation must supervene to give renewed vigor to the parasites. So far as known at the present time, this conjugation takes place only in the digestive tract of the mosquito. That it does actually take place, is undeniable from the observations of MacCallum, Ross, Grassi and others, and the conjugants again as in *Coccidium*, are: a small, motile, microgamete, or male cell (one of the 'flagella' of the *Polymitus* form); and a larger macrogamete, or female, cell (Fig. 3, *p—v*). Their union, observed by Ross and Grassi, takes place in the stomach of *Anopheles*, and the copula then makes its way into, and through, the epithelial cells lining the stomach, and finally rests against the tissue which lines the body cavity. Here it grows to a relatively large size (Fig. 3, *w—zz*), and, when mature, its nucleus divides as in the gregarine or in *Coccidium*, to form a number of spores. Each of these develops a number of germs, or sporozoites, but, unlike the sporozoites of the previously described Sporozoa, these germs have no protective capsule about them, and, when the parent cyst ultimately bursts, they are liberated directly into the body cavity of the mosquito (Fig. 3, A, B). Here, in the fluids of the body cavity, they are carried to all parts of the organism, and finally reach the anterior region of the thorax, where the

salivary glands of the insect are located. They work their way through the cell wall of this gland, into the gland cells, from which they are drawn with the secretion and are finally poured into the lumen of the gland (Fig. 3, C). When the proboscis of the mosquito is inserted into a human host, and the salivary secretion is poured out, the sporozoites pass with it into the blood, and thus effect infection of a new host. Provided with their new potential of vitality resulting from conjugation, the young sporozoites grow and multiply in the blood corpuscles until they are numerous enough to cause the well-known symptoms of malaria. Coming from the same brood, so to speak, they have a similar rate of growth and multiplication, and so liberate their melanin granules throughout the blood system of their human host, at approximately the same time.

The question is frequently asked: Is the mosquito the only agent in the transmission of malaria? and when this is answered by the somewhat modified affirmative, 'Yes, so far as we know,' it is usually followed by the query: 'Why does malaria follow bad drainage, the digging of sewers, laying of gas pipes, etc.?' This question may be answered in two ways: First, it must be shown that these so-called malarial fevers, which accompany such conditions, are in reality true malaria; it is quite possible that hasty diagnosis in many cases gives a wrong impression of the prevalence of this disease. Second, it is conceivable that sporozoites may be carried in the blood, as typhoid is said to be frequently carried in the digestive tract, without causing symptoms of the disease until the natural resistance of the host is weakened by decreased vitality, which may be brought about by bad air, or by other means.

It is quite possible that some other means of transmission than the mosquito exists. The flea, for example, and other insects that prey on man must be examined with this end in view. There is no reason to believe that the sporozoites can be liberated in water, or suspended in the dust of the air, and live, for, of all Sporozoa, the blood-infesting forms are not protected against an external life. Thus we have seen that the sporozoites of the gregarine or of *Coccidium* are incased in a firm, calcareous shell, which protects them from drying and from other dangers that might be encountered. With the malaria-organism there is no such coating; the sporozoites are at all times naked bits of protoplasm, which soon dry up and die, when exposed to the air or placed in water. This fact also refutes the argument made by Bignami and others that the parasites are transferred directly from one individual to another by sticking to the proboscis. It is probable that the mosquito is the original, or primary, host of the malaria-organism, and that man and birds are secondary hosts, from which the parasites return to the primary one for the vital function of conjugation.

THE WILD BIRD AT ARM'S LENGTH: A NEW METHOD OF BIRD STUDY.*

BY PROFESSOR FRANCIS H. HERRICK,

ADELBERT COLLEGE.

IN the study of wild birds the problem of approach has always been difficult to master. The land birds of every continent are, as a rule, shy and difficult to study with that minuteness of detail which alone can satisfy the naturalist and careful observer of their habits.

Birds have enemies to fear and shun, and their discrimination does not exclude their most ardent or curious admirers from their bitterest foes. With them the battle is not always to the strong. Timidity, agility, protective colors and the instinct of concealment are as important in the struggle for life as the bill-hook and mailed foot. We speak of *wild* birds or of *wild animals* generally in contrast to the comparatively few which are tame, and if the wilderness does not always howl, it is often because its inhabitants have found it better policy to remain silent.

Wildness is due to fear which may be inherited or acquired by experience with this wicked world. Tameness on the other hand comes only through the casting out of fear, and may be effected by the formation of new habits, which are either spontaneous or forced. In order to tame a wild animal we must therefore teach it new lessons, and in doing this it is a common practice to literally chain it to a fixed spot, where its conditions of life are uniform and under control, and where no other teachers are allowed to interfere. The moment, however, the wild bird is placed in a cage its behavior is no longer perfectly spontaneous or free, at least not until all fear has been subdued. What is needed, therefore, is an invisible chain to hold the animals to some fixed and chosen spot, which may be approached in disguise.

Fortunately for the student of birds all these conditions are fulfilled for a very important period—that of life at the nest. The *nest* with its *young* is the given fixed point, and *parental instinct* is the invisible chain.

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* Messrs. G. P. Putnam's Sons will shortly publish a book by Professor Herrick, in which the original method described in this paper will be given in greater detail, and the interesting results obtained will be more fully set forth. The book will be entitled 'The Home-Life of Wild Birds: A New Method of Bird Study and Bird Photography,' and will contain upwards of 140 illustrations from nature.—EDITOR.

II.

The method of the study and photography of birds now to be described consists in first bringing the birds to you and then camping beside them. They can thus be watched and photographed at arm's length, or even as near as one would hold a book to read, and under the most perfect conditions of light and position, for hours or days at a time, while quite unconscious of being observed.

FIG. 1. FEMALE CHESTNUT-SIDED WARBLER, BROODING HER YOUNG ON A WARM DAY.

To be more explicit the method depends mainly upon two conditions: (1) The control of the nest or nesting site, and (2) the concealment of the observer.

If the nest like that of an Oriole, Robin, Flycatcher, Waxwing or Vireo is fastened to any leafy branch, the nesting bough or twig is cut off, carefully taken down, carried to a convenient spot where there is

good light, and firmly fastened to stakes driven into the ground. The change is one of space relations which may change with every passing breeze, and though it may be of little significance to the birds it is of

FIG. 2. CEDAR-BIRD, APPROACHING NEST WITH GULLET STUFFED WITH CHERRIES PREPARED TO FEED YOUNG BY REGURGITATION.

the utmost importance to the observer, since the nest now is but four instead of forty or more feet from the ground, and the screen of foliage which hid it from view has been withdrawn.

For an observatory I have adopted a green tent which conceals the student with his camera. The tent is pitched beside the nest, and when in use is open only at one point, marked by a small square window in line with the photographic lens and the nest.

By taking such liberties with wild birds, one might suppose we should bring destruction upon their homes and all that they contain,

but happily this is not the case. No harm need befall either old or young. The former nesting site is soon forgotten, and the new quickly adopted and defended with all the boldness and persistence of which the birds are capable.

This method of studying birds thus depends mainly upon the strength of the parental instincts, and upon the readiness with which a bird learns to adapt itself to new conditions. Upon more complete analysis we recognize the following psychological principles, of which the following are the most important: (a) The strength of an instinct increases through its exercise, and may be reinforced by habit; (b) An

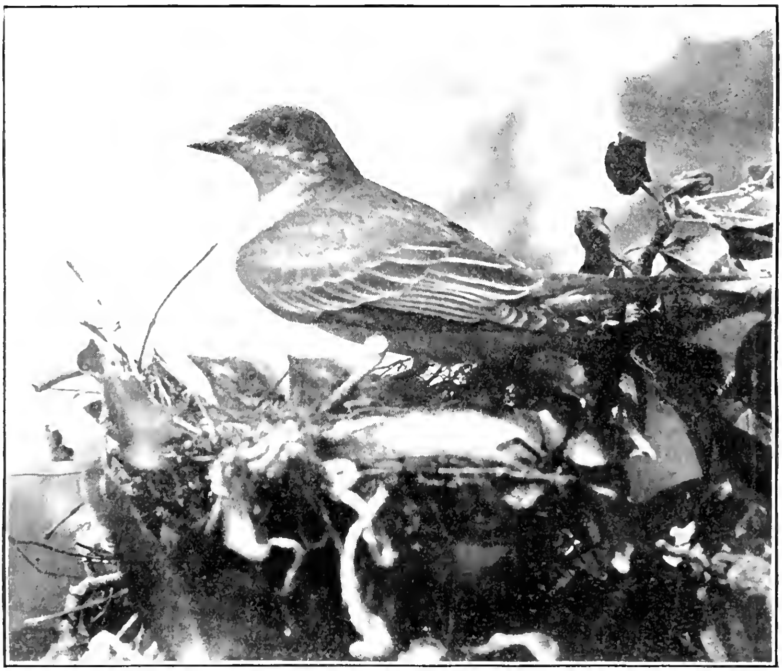

Fig. 3. Female Kingbird, astride Nest, shielding her Young from the Heat.

instinctive impulse may be blocked or suppressed by any contrary impulse; (c) The instinct of fear is often quickly suppressed by repetition, or the formation of new habits. One might also add that: (d) New habits are readily formed and may replace the old ones; (e) Abstract ideas, if they form any part of the furniture of the average bird-mind, are extremely hazy and fleeting; (f) Still further we must recall the physiological fact that birds are guided in most of their operations by sight and hearing, not by scent. Their olfactory organ is very rudimentary at best, and avails them neither in finding food nor in avoiding enemies.

Let us see how these principles are applied to the problem of approaching wild birds in the way described. The parental instincts begin to control the life of the adult with the periodic revival of the reproductive functions, and may vary greatly in their scope and intensity. As a rule this instinct, reïnforced by habit, gradually increases until the young are reared. It is, therefore, safest to change the nesting surroundings when the parental instincts are approaching their culmination.

The general feeling of fear is gradually or quickly suppressed

FIG. 4. KINGBIRDS, BRUISING A DRAGON-FLY BETWEEN THEIR BILLS, AND PREPARING TO SERVE IT TO THE YOUNG. THE FEMALE, WHO WAS BROODING WHEN THE PREY WAS CAUGHT, STANDS AT THE FRONT.

according to the value of the different factors in the equation, by the parental instinct, which impels a bird at all hazards to go to its young wherever placed.

After a bird once visits the nest in its new position it returns again and again, and in proportion as its visits to the old nesting place diminish and finally cease, its approaches to the new position become more frequent, until a new habit has been formed, or, if you will, until the old habit is reinstated.

When the birds approach the nest any strange object, like the stakes which support the bough or the tent which is pitched beside it,

arouses their sense of fear and suspicion, and they may keep away for a time or advance with caution. If very shy, like most catbirds, they will sometimes skirmish about the tent for two hours or more before touching the nest. The ice is usually broken, however, in from twenty minutes to an hour, and I have known a chipping sparrow and red-eyed vireo to feed their young in three minutes after the tent was in place.

At every approach the birds see the same objects which work them

FIG. 5. FEMALE ROBIN CLEANING THE NEST.

no ill. The tent stands silent and motionless, but the young are close by, and fear of the new objects gradually wears away. Parental instinct, or in this case maternal love, for the instinct to cherish the young is usually stronger in the mother, wins the day. The mother bird comes to the nest and feeds her clamoring brood. The spell is broken; she comes again. The male also approaches, and their visits are thereafter repeated.

Possibly the fears of the old birds are renewed at sight of the

window, which is now open in the tent-front, and of the glass eye of the camera gleaming through it, but the lens is also silent and motionless, and soon becomes a familiar object to be finally disregarded. Again there is the fear which the sound of the shutter, a sharp metallic *click*, at first inspires, unless you are the fortunate possessor of an absolutely silent and rapid shutter. At its first report, when two feet away, many birds will jump as if shot, give an angry scream, and even fly at the tent as if to exorcise an evil spirit, while after a few hours they will only wince; finally they will not budge a feather at this or any

FIG. 6. THE BROWN THRUSH, STANDING ON HIS NEST, HAS HEARD AN UNUSUAL SOUND AND IS LISTENING INTENTLY.

other often repeated sound, whether from shutter, steam whistle, locomotive or the human voice.

It is the young, the *young, always the young* in whom the interest of the old birds is centered, and about whom their lives revolve. They

are the strong lure, the talisman, the magnet to which the parent is irresistibly drawn. The tree, the branch, the nest itself, what are these in comparison with the young for whom only they exist? This is true notwithstanding the fact that birds will sometimes leave their young to perish while they start on their migrations. As a rule they follow one line of conduct until their instinct in this direction has been satisfied.

With some species it is possible to make the necessary change without evil consequences when there are eggs in the nest: with others we must wait until the young are from four to nine days old. It is all a question of the strength of the parental instinct, and this varies between wide limits in different species, and very considerably between different individuals. From the nature of the case there can be no infallible rule. If we know little of the habits of the bird in question, it is safest to wait until the seventh to the ninth day after hatching, or when, as in many of the common passerine birds, the feather-shafts of the wing-quills begin to appear, or, better, when they project from one-quarter to one-half inch beyond the feather-tubes. At this period the parental instinct is reaching its maximum, and, what is equally important, the sense of fear has not appeared in the young.

Young birds from one to five days old as a rule cannot stand excessive heat. Even when fed and brooded they will sometimes succumb, and here lies the serious danger to be guarded against. A nest of very young birds well shaded by foliage cannot be safely carried into the direct sunshine of a hot summer's day, hence the importance of beginning operations at the proper time when the weather is suitable, and further of not allowing your enthusiasm to get the better of your judgment.

The young may be handled or fed as much as one wishes, provided they have not acquired the instinct of fear. If you are uncertain as to this, and your aim is to study the nesting habits, it is better to avoid approaching, touching or in any way disturbing the young after the flight-feathers have appeared. The cutting of leaves or twigs which obstruct the light or cast undesirable shadows should be done before this time.

Young birds eight or nine days old stand the heat well, provided they are fed, but on very hot days they should not be allowed to go without food for more than two hours at the longest. Should the parents bring no food during this time, it is better to feed the young in the nest, and to suspend operations until the next day.

The old birds may be expected to come to the nest in from twenty minutes to an hour, when the tent is brought into use immediately after the removal of the nesting bough. It is naturally impossible to predict exactly what will happen until the experiment is tried, since

the personal equation or individuality of the birds themselves is an unknown factor. One thing only is certain: that the parental instinct, reinforced by habit, will win in the end, that it will cast out fear and draw the birds to their young.

III.

I have no desire to anticipate every objection which might be raised against the method, were it possible to do so, but after testing it to the best of my ability with the opportunities of two summers, I am confident of its value and am ready to stand sponsor for it in judicious hands. It is hardly necessary to insist that it is not designed for exhibition purposes, and that its successful practice requires some knowledge, with more patience and time.

An apparently serious objection is likely to occur to the ornithologist, namely, the liability of exposing the birds to new enemies. I feared lest prowling cats should discover the young whose nest and branch had been brought down from the tree-top and set up again in plain sight within easy reach from the ground, but I was happily mistaken. Predacious animals of all kinds seem to avoid such nests, as if they were new devices to entrap or slay them.

As for the weather, barring heat, which must be guarded against in the way described, the nesting bough is more secure when fixed firmly to supports than it could possibly have been before.

The tent not only conceals the observer, but protects his camera, an important consideration, since the prolonged action of the sun is liable to spring a leak in the bellows.

IV.

With note-book in hand you can sit in your tent and see and record everything which transpires at the nest, the mode of approach, the kind of food brought, the varied activities of the old and young, the visits of intruders, their combats with the owners of the nest, the capture of prey which sometimes goes on under your eye. No better position could be chosen for hearing the songs, responsive calls and alarm notes of the birds. You can thus gather materials for an exact and minute history of life at the nest, and of the behavior of birds during this important period. More than this, you can photograph the birds at will, under the most perfect conditions, recording what no naturalist has ever seen, and what no artist could ever hope to portray. The birds come and go close to your eye, but unconscious of being observed.

I have watched the night hawk feed her chick with fireflies when barely fifteen inches from my hand, the kingfisher carrying live fish to its brood whose muffled rattles issued from their subterranean gallery a few feet away. When near enough to count her respirations accurately, I have seen the redwing blackbird leave her nest on a hot

day, hop down to the cool water of the swamp, and after taking a sip, bathe in full view, within reach of the hand; then, shaking the water from her plumage, she would return refreshed to her nest. I have seen the male kingbird come to his nesting bough with feathers drenched from his mid-day bath in the river, the orioles flash their brilliant colors all day long before the eye, and chestnut-sided warblers become so tame after several days that the female would allow you to approach and stroke her back with the hand.

It is difficult to describe the fascination which this method of study affords the student of animal life. New discoveries or unexpected sights wait on the minutes, for while there is a well-ordered routine in the actions of many birds, the most charming pictures occur at odd moments, and there is an endless variety of detail. It is like a succession of scenes in a drama, only this is real life, not an imitation, and there is no need of introducing tragedy.

A STUDY OF BRITISH GENIUS.

By HAVELOCK ELLIS.

VI. MARRIAGE AND FAMILY.

THE tendency to celibacy among men of preeminent intellectual ability has frequently been emphasized by Lombroso and others. It is well illustrated by British men of genius. We may probably assume that by the age of fifty scarcely more than 10 per cent. of the male population remain bachelors, if we take the whole population into consideration. (This is the case in Hungary, and it can not be very far from the truth, so far as Great Britain is concerned.) It is true that, as Körösi and others have shown, among the well-to-do classes men marry both later and seldomer, and that the subjects under consideration largely belong to those classes. We can, however, well afford to leave a margin on this account. We have information concerning the status as regards marriage of 819 of the preeminent men in our list; of these, seventy-two, being Catholic priests or monks (ten of them since the Reformation), were vowed celibates, and 160 others never married. We thus find that 28 per cent. never married, and even if we exclude the vowed celibates, 21 per cent. It must, of course, be remembered that a certain, though not considerable, proportion of the unmarried were under fifty at death, and some of these would certainly have married had they survived. It may be added that about two-thirds of the women were married, though several of those (especially actresses) belonging to the unmarried third formed *liaisons* of a more or less public character, and in a few cases had several children.

It must not be supposed that all these eminent men who lived long lives in celibacy were always so absorbed in intellectual pursuits that the idea of matrimony never occurred to them. This was not always the case. Thus we are told of Dalton, that the idea had crossed his mind, but he put it aside because, he said, he 'never had time.' In several cases, as in that of Cowley, the eminent man appears really to have been in love, but was too shy to avow this fact to the object of his affections. Reynolds is supposed only once to have been in love, with Angelica Kauffmann; the lady waited long and patiently for a declaration, but none arrived, and she finally married another; Reynolds does not appear to have been overmuch distressed, and they remained good friends. These cases seem to be fairly typical of a certain group of the celibates in our list; a passionate devotion to intellectual pursuits seems often to be associated with a lack of passion

in the ordinary relationships of life, while excessive shyness really betrays also a feebleness of the emotional impulse. Even in many cases in which marriage occurs, it is often easy to see that the relationship was rooted in the man's intellectual passion.*

The average age of marriage among the men in our list, taking one hundred cases, is found to be thirty-one years, the most frequent age being from twenty-eight to thirty-two inclusive. Of these, four were under twenty, and thirteen over forty at the date of their first marriage. This proportion of late marriages is abnormally high, especially when we remember that the marriages of widowers are here excluded. The proportion of early marriages is somewhat low, as compared with the general population in England to-day. The average age, thirty-one years, is distinctly late, more especially when we remember that it only includes first marriages. The average age of marriage for all males during recent years in England is between twenty-eight and twenty-nine years, and at the other side of the world, in New Zealand, though later, it is still below thirty. The most frequent age of marriage also falls much earlier. In estimating the significance of these figures as regards men of genius, we have to remember, on the one hand, that the well-to-do classes, to which men of preeminent intellectual ability largely belong, marry later than the general population, and, on the other hand, that the general tendency to marry late is of recent growth. If we are entitled to believe that these conflicting tendencies balance each other, the data still indicate that British men of genius have shown a tendency not only to marry seldom, but to marry late.

The married women on our list form too small a group to generalize about with safety. One notable fact, however, emerges. They show a tendency to marry either before or after the period at which the majority of married women marry, but not during that period. In England during recent years the average age at which women marry has been about twenty-six years. But among British women of genius very few marriages take place during the period of great reproductive energy; the large majority of such marriages fall outside the period between twenty-three and thirty-four years of age. In the majority of cases marriage took place before this period, the relationship, from one reason or another, being very often dissolved not long afterwards; but in a very considerable proportion of cases, marriage never took

* Dr. P. Garnier ('Célibat et Célibataires,' pp. 72-5) has some interesting remarks on this point. He considers that genius is, or should be, celibate, and that a man of genius is not usually able to make a woman's life happy. He mentions that among the eighty-four professors at the medical faculties of Paris, Lyons and Bordeaux—the three chief medical centers of France—fifteen are celibates, and of the sixty-nine who are married eleven are childless.

place until after this period. Thus, Fanny Burney married at forty-one, Mrs. Browning at forty, Charlotte Brontë at thirty-eight, while George Eliot's relationship with Lewes was formed at about the age of thirty-six; these names include the most eminent English women of letters. It would thus appear that there is a tendency for the years of greatest reproductive activity to be reserved for intellectual development, by accelerating or retarding the disturbing emotional and practical influences of real life. This tendency might still be beneficial, even when the best work was not actually accomplished until after a late marriage.

We have now to consider the fertility of the marriages formed by men of preeminent intellectual ability. Lombroso and others have insisted on the tendency to sterility among men of genius, but have always been content merely to cite a few cases in proof. This method can at the most raise merely a presumption in favor of the dictum laid down. The present investigation, covering a very large group of men of the highest intellectual eminence, furnishes more conclusive evidence as to the actual facts. It confirms only to a limited extent the belief in the relative sterility of men of genius, though we have to remember the very high mean age of the individuals we are considering. The married men of intellectual ability in our list number 587; of these, 448 had children; seventy-six are definitely stated by the national biographer not to have had children; sixty-three cases remain in which the point is passed without mention, or in which it is stated that the marriage was not fruitful, but that there were illegitimate children. It appears, so far as I can judge, that in the majority of the sixty-three doubtful cases, there were really no legitimate children; this has most often been found to be the case when I have checked the national biographer by other sources of information. In a certain proportion of cases, however, the facts regarding children are not known, and in others the children have apparently been ignored. We may probably conclude that nearly two-thirds of these sixty-three doubtful cases were really unfruitful. (I may add that, even if we exclude the doubtful cases altogether, the proportion of unfruitful marriages remains very abnormally large.) We then find that about 20 per cent. of the marriages of British men of genius have been unfruitful. In this case we have not much difficulty in obtaining a normal standard of comparison. Karl Pearson, manipulating the data furnished by Howard Collins, has found that during the past century among the middle and upper classes chiefly of British race, or belonging to the United States —a class fairly comparable to those in the present group—the total sterility is about 12 or 13 per cent., rather less than half of this (*i. e.*, about 6 per cent.) being due to what is termed 'natural sterility'; while the remainder (*i. e.*, 6 or 7 per cent.) must be set down to artificial restraints on reproduction. If, again, we turn to New Zealand, where

the methods of death registration enable us to form an approximate estimate of the proportion of childless marriages among the whole population of somewhat mixed British race, with a high standard of living, we find that the proportion of marriages in which there are no surviving children at the father's death is about 16 per cent. With due allowance for the earlier death of the children and for the ignorance, in a certain proportion of cases, of those who filled in the death certificate, it is probable enough that this result is not really larger than the other. In any case there is an excess of sterility among the group of intellectually eminent men, this excess being the more marked when we remember that in very large majority they belong to a period when the artificial restraint of reproduction had scarcely begun to be widely practiced.

It is somewhat remarkable that, although the number of infertile marriages is so large, the average fertility of those marriages which were not barren is by no means small. We have fairly adequate information in the case of the marriages of 214 of these eminent men. I have not included those cases in which the national biographer is only able to say that there were 'at least' so many children, nor have I knowingly included any cases in which there were two or more marriages. Whether the number of children represents gross or net fertility, it is, unfortunately, in a very large proportion of instances, quite impossible to say. It is probable that in a large proportion of cases only the net fertility, *i. e.*, the number of children who survived infancy and childhood, has been recorded. It is, therefore, the more remarkable that the average number of children in these 214 fertile families is 5.45. Thus, although our data are probably imperfect, they show that the fertile marriages of British men of genius have produced families which contain on the average one child more than the fertile marriages of ordinary people of the same race during the nineteenth century; in New Zealand the average number of children left by fathers of families (whether as the result of one or more marriages) dying between the ages of twenty-five and sixty-five, is 4.81, which indicates a much larger gross fertility. It must, of course, be remembered, on the other hand, that the eminent men in our group lived to a very high average age, and it is obvious that men who live to an advanced age will have a better chance of leaving large families than those who die young.* This consideration somewhat diminishes our estimate of the fertility shown by British men of genius, while, if we take barren marriages into

* Even apart from this, there appears to be a connection between longevity and fecundity; see M. Beeton and Karl Pearson, 'On the Correlation between Duration of Life and the Number of Offspring,' a paper presented to the Royal Society of London, June 14, 1900.

account, the fertility is greatly pulled down, but still remains well up to the average. This normal average is thus attained by a conjunction of an abnormal proportion of sterile marriages, together with an abnormally high proportion of children among the fertile marriages.

There would appear to be a considerable resemblance between the fertility of genius families and of insane families. We have seen previously that our eminent British persons belonged to families of probably more than average fertility; we now see that they themselves produced families of probably not more than average size, owing to a greater prevalence of sterility. In France, Ball and Régis, confirmed by Marandon de Montyel, appear to have found reason for a similar conclusion regarding the insane. They state that natality is greater among the ascendants of the insane than in normal families, but afterwards it is the same as in normal families, while they also note the prevalence of sterility in the families of the insane.* The question, however, needs further investigation.

With regard to the distribution of families of different sizes, the results, as compared with the figures already given, are as follows:

Size of Family	1	2	3	4	5	6	7	8
Normal Families	12.2	14.7	15.3	14.1	11.1	8.6	7.8	6.3
Genius-producing Families	6.2	6.2	11.0	8.4	10.6	10.2	11.7	6.9
Families of Men of Genius	12.6	14.5	10.3	13.6	8.8	6.5	10.3	4.7

Size of Family	9	10	11	12	13	14	over 14
Normal Families	3.9	2.7	1.4	1.0	.5	.2	.1
Genius-producing Families	5.5	4.4	5.8	4.0	2.9	1.8	4.0
Families of Men of Genius	5.1	2.8	3.2	.9	2.3	1.4	2.3

Allowing for certain irregularities due to the insufficient number of cases, the interesting point that emerges is the return towards the proportions that prevail in normal families; it will be seen that in all but a few cases the families of men of genius differ from genius-producing families by approximating to normal families. It must be remembered that in neither of our groups are the data absolutely perfect, but as they stand they confirm the conclusion already suggested that men of genius belong to families in which there is a high birth-rate, a flaring up of procreative activity, which in the men of genius themselves subsides towards normal proportions. The slightly larger average size of the families of men of genius as compared with normal families is merely due to the presence of a few families of excessively large size.

* See, for a summary of these results, Toulouse, 'Les Causes de la Folie,' p. 91; 1896.

It will be noticed that the families of sizes ranging between three and six, both inclusive, are unduly few. It might be supposed that this is due to the artificial limitation of families, more especially since, as Karl Pearson has pointed out, in the normal families themselves there is already a deficiency in those groups, probably due to this cause. I am, however, inclined to doubt whether that is so in the case of families of men of genius, although to some extent it may be so. There seems some reason to suppose that from the present point of view the group may not be homogeneous, but made up in part of men with feeble vitality and a tendency to sterility, and in part of men with a tendency towards unusual fecundity, thus leading to a deficiency of medium-sized families.

In the case of 147 families of men of genius, it has been possible to ascertain the number of children of each sex. This is found to be 100 girls to nearly 103 boys. This is almost the normal proportion of the sexes at birth at the present time in England. If, however, I am right in supposing that in a certain proportion of our cases the biographers have stated not the gross fertility, but only the net fertility (or the surviving children), we are not entitled to expect so close an approximation to the proportions at birth, since the preponderance of boys begins to vanish immediately after birth. The figures thus suggest that the families of men of genius show the same tendency to excess of boys, which we have already seen to be clearly marked in the case of the families producing men of genius. The data are too few to indicate whether there is any corresponding excess of girls in the families of women of genius.

VII. DURATION OF LIFE.

It has long been a favorite occupation of popular writers on genius to estimate the ages at which famous men have died, to dilate on their tendency to longevity, and to conclude, or assume, that longevity is the natural result of a life devoted to intellectual avocations. The average age for different groups, found by a number of different inquirers, varies between sixty-four and seventy-one years. One writer, who finds this highest age for certain groups of eminent men of the nineteenth century, argues that here we have a test from which there is no appeal, proving the preeminence of the nineteenth century over previous centuries, and its freedom from 'degeneration.' It did not occur to this inquirer to ask at what age the famous men of earlier centuries died. I have done so in the case of a small group of ten eminent men on my list, dying between the fourth and the end of the thirteenth centuries—including, I believe, nearly all those in my list of whose dates we have fairly definite information during this period—and I find that their average age is exactly seventy-four years. So

that, if this test means anything at all, the freedom of the nineteenth century from 'degeneration' is by no means proved.

In reality, however, it means nothing. If genius were recognizable at birth there would be some interest in tracing the course of its death-rate. But it must always be remembered that when we are dealing with men of genius, we are really dealing with *famous* men of genius, and that though genius may be born, fame is made—in most fields very slowly made. Among poets, it has generally been found, longevity is less marked than among other groups of eminent men, and the reason is simple. The qualities that the poet requires often develop early; his art is a comparatively easy one to acquire and exercise, while its products are imperishable and of so widely appreciated a character that even a few lines may serve to gain immortality. The case of the poet is, therefore, somewhat exceptional, though even among poets only a few attain perfection at an early age. In nearly every other field the man of genius must necessarily take a long period to acquire the full possession of his powers, and a still longer period to impress his fellowmen with the sense of his powers, thus attaining eminence. In the case of the lawyer, for instance, the path of success is hemmed in by tradition and routine, every triumph is only witnessed by a small number of persons, and passes away without adequate record; only by a long succession of achievements through many years can the lawyer hope to acquire the fame necessary for supreme eminence, and it is not surprising that of the thirty-four preeminent lawyers on my list only four were under sixty at death. Much the same is true, though in a slightly less marked degree, of statesmen, divines and actors.

It is, therefore, somewhat an idle task to pile up records of the longevity of eminent men of genius. They live a long time for the excellent reason that they must live a long time or they will never become eminent. It is doubtless true that men of genius—mostly belonging to the well-to-do classes, and possessing the energy and usually the opportunities necessary to follow intellectual ends of a comparatively impersonal and disinterested character—are in a far more favorable position for living to an advanced age than the crowds who struggle more or less desperately for the gratification of personal greeds and ambitions, which neither in the pursuit nor the attainment are conducive to peaceful and wholesome living. This may well be believed, but it is hardly demonstrated by the longevity of eminent men.

At the same time it is of some interest to note the ages of the eminent persons on our list at death. Though the facts may have little significance in themselves, they have a bearing on many of the other data here recorded. Excluding women, and including only those men whose dates are considered by the national biographers to be unquestionable, the ages of eminent British men at death range from Chatter-

ton, the poet, at seventeen, to Bishop Morton, the scholar (born in the seventeenth century), and Sir Edward Sabine, the man of science (born in the eighteenth century), at ninety-five. They are distributed as follows in five-year age-periods:

Age at Death...	Under 20	20–24	25–29	30–34	35–39	40–44	45–49	50–54
Men of Genius..	1	2	5	13	13	29	48	51

Age at Death.	55–59	60–64	65–69	70–74	75–79	80–84	85–89	90 and over
Men of Genius	66	84	108	116	86	49	35	14

If we consider the number for each year separately, certain points emerge which are disguised by the five-year age-period, though the irregularities become frequently marked and inexplicable. A certain order, however, seems to be maintained. There is scarcely any rise from twenty-seven to thirty-eight, and even at forty-five only three individuals died; but, on the whole, there is a slow rise after thirty-eight, leading to the first climax at forty-nine, when sixteen individuals died; this climax is maintained at a lower level to fifty-four, when there is a marked fall to a level scarcely higher than that which prevailed between the ages of forty-one and forty-three. This lasts for three years; then there is a sudden rise from seven deaths at fifty-six, to twenty-five deaths at fifty-seven, and this second climax is again maintained at a somewhat lower level to the age of sixty-seven, when the highest climax is attained, with thirty-one deaths. Thereafter the decline is slow but steady, with a final climax of twenty deaths at seventy-eight. It is curious that each climax is sudden, and preceded by a fall.

A noteworthy point here seems to be the very low mortality between the ages of fifty-three and fifty-seven. It seems to confirm Galton's conclusion, based on somewhat similar data, that a group of men of genius is in part made up of persons of unusually feeble constitutions and in part of persons of unusually vigorous constitutions. After the first climax at forty-nine the feeble have mostly died out. The vigorous are then in possession of their best powers and working at full pressure; fifty-seven appears to be a critical age at which exhaustion and collapse are specially liable to occur. The presence of these two classes—the abnormally weak and the abnormally vigorous—would be in harmony with the explanation I have already ventured to offer of the deficiency of medium-sized families left by our men of genius.

The age of the women is ascertainable in thirty-nine cases. The average is extremely high; four died before forty, but nine lived to over eighty, and two of these were over ninety.

THE PROGRESS OF SCIENCE.

THE NATIONAL ACADEMY AND OTHER SCIENTIFIC SOCIETIES.

THE annual stated meeting of the National Academy of Sciences was held in Washington in the third week in April. Professor Wolcott Gibbs, one of the two surviving founders of the Academy and the distinguished dean of American men of science, having resigned the presidency a year ago, Mr. Alexander Agassiz, of Cambridge, was elected to the office. It may almost be said that Mr. Agassiz assumed the presidency by right, as he exactly represents the hereditary distinction and aristocratic preeminence of a small and select National Academy. It is possible that such an institution belongs to the past rather than to the democracy of the twentieth century, but there is perhaps less danger in America from the preservation of precedents than from their abolition. Mr. Asaph Hall remains vice-president and Mr. Charles D. Walcott treasurer of the Academy, while the vacancy in the foreign secretaryship, caused by Mr. Agassiz's elevation to the presidency, was filled by the election of Prof. Ira Remsen, whose former office of home secretary is now occupied by Mr. Arnold Hague. Five new members were elected: George F. Becker, U. S. Geological Survey, Washington, D. C.; J. McKeen Cattell, professor of psychology, Columbia University, New York City; Eliakim H. Moore, professor of mathematics, University of Chicago, Chicago, Ill.; Edward L. Nichols, professor of physics, Cornell University, Ithaca, N. Y., and T. Mitchell Prudden, professor of pathology, College of Physicians and Surgeons, Columbia University. An improvement has recently been made in the manner of election to the Academy. The members have been divided into six standing committees, and a nominee must be endorsed by the committee having an expert knowledge of his qual-
having an expert knowledge of his qual-
1899 only thirteen new members were elected, although twenty-seven vacancies occurred through death, whereas during the past three years fourteen new members have been elected. Eight foreign associates were elected: MM. Janssen, Loewy, Bornet and Cornu, of France; Professors Kohlrausch and van't Hoff, of Germany; Professor Kronecker, of Switzerland, and Sir Archibald Geikie, of Great Britain. The Henry Draper medal was awarded to Sir William Huggins, president of the Royal Society, for his investigations in astronomical physics. In the scientific sessions of the Academy eleven papers were presented, as follows:

'The Climatology of the Isthmus of Panama': Henry L. Abbot.

'The Effects of Secular Cooling and Meteoric Dust on the Length of the Terrestrial Day': R. S. Woodward.

'The Use of Formulæ in demonstrating Relations of the Life History of an Individual to the Evolution of its Group': Alpheus Hyatt.

'Artificial Parthenogenesis and its Relation to Normal Fertilization': E. B. Wilson.

'Simultaneous Volumetric and Electric Graduation of the Condensation Tube': Carl Barus.

'Table of Results of an Experimental Enquiry regarding the Nutritive Action of Alcohol, prepared by Prof. W. O. Atwater, of Middletown, Conn.': Presented by J. S. Billings.

'The Significance of the Dissimilar Limbs of the Ornithopodous Dinosaurs': Theo. Gill.

'The Place of Mind in Nature,' 'The Foundation of Mind': J. W. Powell.

'Conditions Affecting the Fertility of Sheep and the Sex of their Offspring': Alexander Graham Bell.

'The New Spectrum': S. P. Langley.

DURING the same week that our National Academy was meeting at Washington, the recently established Interna-

tional Association of Academies was holding its first regular session at Paris. This association was organized at Wiesbaden two years ago and held a preliminary meeting at Paris last year. It is composed of representatives of the great academies of the world—eighteen in all—and includes in its scope literature as well as science. The object of the Association is to promote international cooperation in scientific work; it represents a movement the importance of which is very great and the accomplishment of which is very difficult. It is probable that a more representative congress would better forward the ends in view, but it may be that such a congress can best be developed by beginning with a small meeting of eminent men. There seems, however, to be good reason to protest against the star chamber methods which the Association seems inclined to adopt. We are told of a dinner given by the municipal council of Paris, a reception by the president of the French Republic and a theatrical performance at the Comédie Française, but not the slightest information can be obtained regarding the secret sessions of the Academy, at which scientific plans were presumably considered. Questions to be taken up should be announced well in advance, and they should be carefully considered by scientific men and discussed in the scientific press. M. Darboux was acting-president for the meeting, and the honorary presidents were Dr. Mommsen, Dr. de Goeje, Sir Michael Foster, M. Berthelot and M. Gaston Boissier. We regret to learn that the delegate from the National Academy of Sciences, Prof. G. L. Goodale, was detained by illness at Geneva; otherwise all the academies were represented. The next meeting will be in London in 1904.

The Council of the American Association for the Advancement of Science held its spring meeting at Washington at the time of the sessions of the National Academy of Sciences. The report made by the permanent secretary was very gratifying. Over seven hundred new members have been elected within the past year; the membership is now larger than ever before and includes almost the whole body of scientific workers in this country. The sum of $1,300 has been saved from current receipts and turned over to the permanent fund for the encouragement of research. The arrangement by which the weekly journal 'Science' is sent free of charge to members is apparently giving perfect satisfaction and is helping the Association in many ways. Progress has been made towards securing an agreement among universities and other institutions to set aside a week after the Christmas holidays for the meetings of scientific and learned societies. The plan was unanimously approved by the Association of American Universities, and Columbia and Cornell have already taken action lengthening their vacations for this purpose. Progress was reported in the arrangements for the Denver meeting, a single fare on the railways west of Chicago having been secured. The meeting, the first to be held so far toward the West, promises to be of unusual importance. Readers of this journal who are not members of the Association may obtain information as to the conditions of membership by addressing the permanent secretary, Dr. L. O. Howard, the Cosmos Club, Washington, D. C.

THE HARVARD COLLEGE OBSERVATORY.

The fifty-fourth annual report of the director of the Harvard College Observatory gives a clear idea of the activity which prevails at that institution. As the present year marks the beginning of a new century, Professor Pickering finds the time opportune for describing the present condition and needs of the Observatory. As stated in the previous annual report, the invested funds amount to something over $800,000. The annual income is about $50,000, but, owing to the continued diminution

in the rate of interest on invested property, the income will steadily decrease unless an additional sum is obtained. Two hundred thousand dollars more are needed to place the institution in such a condition that its standing among the great observatories of the world shall be secure. The buildings at Cambridge are old and to provide new ones, suited to modern requirements, will cost at least $100,000. The present buildings at Cambridge are valued at $52,000 and at Arequipa $12,000. The instruments at Cambridge are valued at $20,000 and at Arequipa $50,000. The great need of the Observatory in the instrumental way is a great telescope for the southern hemisphere, the cost of which, for construction and maintenance, would be about $200,000. Altogether, half a million dollars are desired to make the Observatory worthy of the great future which opens before it. More money is also needed for publication. Already there have been issued by the Observatory about forty quarto volumes of the 'Annals,' embracing researches in many lines of astronomy and meteorology. An enormous amount of material, however, is still awaiting publication, sufficient to make about twenty-eight additional volumes. Some of these will be issued soon, and all as rapidly as the nature of the work and the funds available for publication will permit.

During the last year several lines of investigation have been pursued. Photometric and photographic determinations were made of the brightness of a great number of stars, including several hundred variables. The reduction of the observations of the zones made in former years with the meridian circle has been carried forward. As usual, the whole sky was photographed several times on a small scale. These photographic charts have proved of the greatest value in tracing the past history of new stars, variable stars and special new objects, such as the little planet Eros. Also a very large number of photographs have been made of special objects with instruments of greater power. Progress was made in the study of the spectra of the stars and several objects of special interest were discovered, including one nova. Intimately associated with the institution is the Blue Hill Meteorological Observatory—where during the year some striking experiments were carried on in kiteflying. A meteorograph, suspended under the kite, gave records at heights as great as 15,800 feet above sea level. In Peru, the line of meteorological stations extending from the Pacific across the Andes, with one on the summit of El Misti, at an elevation of 19,200 feet, has been maintained. The Harvard Observatory acts as the distributing center in this country for all telegraphic announcements of astronomical discoveries. During the year twenty messages were sent out to American and European astronomers.

THE NEW YORK BOTANICAL GARDEN.

The advance sheets of the 'Bulletin' of the New York Botanical Garden for 1901, and the pages of the 'Journal,' show most gratifying progress in that institution since the preparation of the article dealing with it published in this magazine in June of last year. About seven thousand species of plants are now successfully cultivated in the open air and under glass. The large conservatories, which were completed in 1900, have been filled by plants received as gifts and as exchanges. Many donations of great value have been received from various persons, and notable exchanges have been made with the Buffalo Botanic Garden and Fairmount Park. A collection of succulents, numbering about five hundred species, purchased by Dr. N. L. Britton during his recent European tour in attendance at the International Congress of Botanists in Paris, has been recently received and is now installed in the conservatories. Mr. Samuel Henshaw resigned from the position of head gardener on January 1, 1901, and was sent on a collecting tour in the West Indies. He has recently re-

turned with valuable material from Trinidad and Jamaica. Mr. George V. Nash, who was promoted from the position of curator of the plantations to the place of head gardener, spent February and March at the Royal Gardens, Kew, by special invitation, for the purpose of selecting from the duplicates of the immense collections of living plants. About two thousand species were secured, the greater number of which promise to thrive, and form the most valuable single addition yet made to the flora of the Garden. Mr. Percy Wilson, museum aid, was sent with the Amherst Astronomical Expedition to Sumatra in March and will spend about six months in that region securing specimens for the economic museum and living plants for the horticultural houses. Other explorations are projected for the present season.

A set of propagating houses was erected at a cost of $16,000 in the latter part of 1900, some new road and pathways built to it, and other ground improvements made. The New York Central and Hudson River Railroad has completed a station on the margin of the grounds, which enhances the beauty of one of the principal approaches to the Museum. Contracts aggregating nearly $200,000 have been let for the present season, embracing the completion of the main horticultural houses, main approaches and grounds in front of the Museum, fountains, roadways and areas round and near both buildings. The income of the Garden from all sources amounts to over $75,000 for 1901. The library was increased by over fifteen hundred volumes and a large number of separates during the year 1900. The herbarium received additions amounting to about seventy thousand specimens, inclusive of the Morong Herbarium of Barnard College, which is deposited under the same conditions as that of Columbia College. Dr. T. F. Allen, the noted specialist on the Characeæ, has recently given his collection of that group to the Garden without reserve, and it is now in process of arrangement under Dr. Allen's supervision. The economic museums have been filled out in many important particulars, but remain in a skeleton state, as a number of years will be necessary to make an adequate representation of many of the subjects taken up. The exhibition microscopes installed a year ago have been objects of great interest and profit to visitors.

The laboratories have accommodated twenty-eight investigators during the year, and the results of some of their researches have been published as contributions, or are being offered as theses, by candidates for degrees at various universities. These investigations extend over the entire range of botany. The equipment has been steadily increased to meet the varied needs of these workers, and the experimental greenhouses afford valuable supplemental facilities in such work. In addition to these original researches, Dr. N. L. Britton has finished a 'Manual of the Plants of Eastern United States,' which is being published by the Henry Holt Company, and Dr. D. T. MacDougal has written an advanced text-book of 'Practical Plant Physiology,' published by Longmans, Green & Co. Seventeen popular lectures were given in the lecture hall of the Museum in the winter of 1900-1901, which were attended by one to five hundred people. The annual meeting of the Horticultural Society of New York was held at the Garden, May 8 and 9, 1901, and the exhibition was notably successful.

THE PRACTICAL APPLICATIONS OF ELECTRICITY.

In the *Electrical World and Engineer* for January 5, 1901, the first number of the twentieth century, appeared a series of articles on the past progress of applied electricity and upon its prospects for the future. Among the authors are some of the most prominent of American electricians, such as Elihu Thomson, A. E. Kennelly, Louis Bell, Kempster B. Miller, Carl Hering, H. Ward Leonard, Patrick B. Delany, and some of the

more prominent men in science. The various papers are quite largely devoted to statements of the unsolved practical and theoretical problems, in so far as they are capable of statement.

In the line of theory and research the most promising field seems to be the development of the *electron theory* which has been in the past mainly built up on experimental studies of the electrical discharge in gases. This theory is an attempt to represent all electrical phenomena in terms of the conception of the *electron*, an excessively minute charged particle, a thousand times smaller than an atom. This theory has already given a remarkably clear insight into the electrical properties of gases, and tentative explanations of the most promising kind of the ultimate constitution of matter, and of the nature of gravitation, two of the most stupendous problems of the present day in physics.

Among the purely practical problems, purely practical because the scientist has, one might almost say, given it up in despair, is the production of electrical energy direct from coal. One can say almost to a certainty that if one is to transform a large percentage of the latent energy of coal into electrical energy the coal *must not be burned*. The mischievous waste at present endured seems to be inherent in the burning process, but the wildest dreamer has never imagined that coal can be of any use other than for feeding a fire! There does seem to be a difficulty here, and perhaps the example of the scientist who revels in thermodynamics might just as well be followed by the inventive practical man. One thing, however, is certain and that is that the solution of this problem depends upon the invention of a new kind of fire which can be stopped in mid air, as it were, by a turn of the hand, each atom of carbon and each atom of oxygen stopping its mad whirl to begin it again at our pleasure, standing in the meantime in a state of quiet expectancy. Such a fire the physicist would call a *reversible* fire. The burning of zinc in a voltaic cell is indeed such a fire, and most of the attempts to transform the latent energy of coal into electrical energy efficiently have been attempts to construct a voltaic cell which will burn coal. Another kind of reversible fire might be realized in the gas engine if we had materials, to build a gas engine of, which would stand excessively high temperatures and excessively high pressures. In such a gas engine a mixture of gas and air could be enormously compressed and made so hot that it could not burn, then by expanding the mixture combustion would slowly take place and in such a way that to stop expanding would be to stop the combustion. Such combustion would be reversible, for to recompress the mixture would literally unburn it. Such a gas engine would have a very high efficiency if one could keep the cylinder from being cooled by the surrounding air.

The burning of food in the body of a work horse is a case in which an unusually large percentage of the latent energy of the fuel, or food, is converted into useful work, and thermodynamics tells us beyond peradventure that this high efficiency must be due to a state of affairs something like the following: Let us imagine the muscles built up of enormously complicated molecules, like the molecules of albumin for example, and let us imagine that as a muscle contracts these complicated molecules are distorted slowly, and that as they become distorted some of the atoms of carbon and hydrogen are slowly bulged out of the molecular structure and gingerly allowed to approach the atoms of oxygen in the blood in such a way that the process would be arrested at any moment by a cessation of the contraction. If such a process could be completely realized and if the atoms of oxygen could also be kept at bay by being themselves involved in some fashion in the bulging process of the muscular structure, then the efficiency of muscular action would be one hundred per cent. It is in fact much less

than one hundred per cent., on account, perhaps, of the oxygen molecules being unbridled.

THE WISCONSIN AGRICULTURAL EXPERIMENT STATION.

The seventh annual report of the Wisconsin Agricultural Experiment Station is a notable example of a class of literature which is rapidly increasing in extent and in popular interest. The reports of this station have become widely known for the important contributions which they contain and for their interest to those who follow the progress of science, as well as to the progressive farmer, whose needs are kept constantly in view. The last report, which is for the year ending June 30, 1900, is fully up to the standard of previous reports. It adds another chapter to the interesting studies on the process of cheese ripening or curing which Dr. Babcock and Dr. Russell have been conducting for a number of years. In the past their studies have led to the discovery of a natural enzym in milk which has been shown to be an active agent in the digestion of the proteids of cheese, rendering them soluble and digestible. This discovery, which was contrary to the prevalent bacterial theory and was opposed by many bacteriologists, has stood the test, and the theory is now generally believed in, the main point at issue being the extent and the exact character of the changes which the ferment induces. The present report takes up the action of another ferment in cheese, namely, rennet, which is added during the process of manufacture. There has been much diversity of opinion as to whether the rennet had any part in the ripening process, but very little real investigation. The most recent writings on the subject have disclaimed any action due to rennet. Babcock and Russell now show that rennet undoubtedly assists in peptonizing the casein of cheese, the active agent being the peptic enzyms contained in the rennet extracts. The investigation is a quite comprehensive one, involving a study of the action of pure pepsin as compared with rennet, the conditions of the acidity most favorable to the action, and the nature of the products. The matter is of considerable importance, since it is found that the amount of rennet used influences the rapidity and the thoroughness of the ripening process. The wonder is that a point upon which there has been such marked discrepancy of opinion has not been given a thorough investigation before this.

Of equal interest is the investigation of the cause and character of the changes in green fodder when preserved as silage in the silo, also by Drs. Babcock and Russell. The generally accepted theory of silage formation as fermentation changes, due to the action of bacteria and molds, is found to be erroneous, inasmuch as good silage was made under conditions which positively precluded bacterial activity, i. e., in the presence of anæsthetics. With the aid of an ingeniously-devised closed-circuit respiration apparatus, the authors were able to study the gaseous products and to maintain the conditions entirely under their control. The unavoidable losses in ensiling green fodder were found to be due to the formation of water, carbon dioxid and volatile organic acids, which are produced, not as a result of the bacterial action, but as a result of the intromolecular respiratory processes of the plant tissues. The avoidable losses, on the other hand, are found to be due mainly to the decomposition of organic matter induced by the development of bacteria and molds, whose growth is greatly facilitated by the admission of air, as a result of improper construction of the silo. The bacteria are, therefore, instead of being essential to good silage, only deleterious. In view of the extent to which silage is now prepared in this country, and the fact that the spoiled or partly spoiled silage is not only a loss but is likely to be injurious to stock, these results, which furnish a clearer under-

standing of the factors which enter into the process, cannot fail to be of much practical importance. A study of the thermal death point of tubercle bacilli in milk confirmed the results of the previous year, showing that by using a closed pasteurizer tubercle bacilli can be destroyed by heating milk for twenty minutes at 140° F., instead of at 150-155°, as formerly. This lowering of the necessary temperature removes the objection formerly made that cream does not rise readily on pasteurized milk and that the consistency or body of pasteurized cream is much lessened.

The soil investigations of the Wisconsin Experiment Station have come to be regarded as one of its most prominent features. In addition to investigations on the soluble salts of cultivated soils, the influence of potash salts on the black marsh soils of the State, which have been exceedingly difficult to manage, Professor King reports studies of the influence of the right amount and the right distribution of water in crop production—a subject upon which information is quite meager and which points directly to the application of irrigation, even in humid climates, to correct insufficient or inadvantageously distributed rainfall. In the horticultural line, Professor Goff has for several years been studying the injury to the buds and roots of fruit trees from cold, and in the present report he gives an illustrated account of his investigations on the time of formation and the development of the flower buds, and also on the resumption of root growth of fruit trees in the spring; and his assistant gives the results of systematic observations on the duration of the growth period in fruit trees. These subjects are closely connected with the management of fruit orchards and will furnish a rational basis for practice. In addition to these lines of investigation, the report also contains accounts of feeding experiments with pigs, sheep and dairy cows, to answer more immediately practical questions; studies of various factors influencing the Babcock milk test, a rapid method for the estimation of salt in butter, a trial of a new kind of churn, operated by forcing a steady stream of air into the cream, for which great claims have been made; studies of the effect of the continued use of immature seeds; studies relating to tannery refuse and hides as causes of the disease in animals known as anthrax; experience with sugar beet culture in Wisconsin, and several other lines.

Altogether, the report is one of surpassing interest, and the results of quite a part of the experiments and investigation will not be confined in their application within the borders of the State. If any indication is needed of the importance of the work being done by the agricultural experiment stations of this country and the wisdom of Congress in continuing liberal appropriations for their work, this report should go a long way in the direction of furnishing such indication.

SCIENTIFIC NEWS.

Science in America could have suffered no more severe loss than the death of Henry Augustus Rowland, professor of physics in the Johns Hopkins University. Dying at the age of fifty-two years, he was one of the world's most eminent men of genius and one of the two great physicists that America has produced. An account of Rowland's life and work, with a portrait, was published in the Popular Science Monthly for May, 1896, and we reproduce in the current number an address given by him some years ago before the American Association for the Advancement of Science.

We note with regret a number of other recent deaths among men of science, including: Thomas Conrad Porter, the botanist, for the past thirty-four years professor at Lafayette College; John Thomas Duffield, for more than forty years professor of mathematics at Princeton University: Richard T. Rothwell, editor of the 'Engineer-

ing and Mining Journal'; Frederick J. Brockway, assistant demonstrator of anatomy at the College of Physicians and Surgeons of Columbia University; F. M. Raoult, the eminent chemist, professor at Grenoble; Paul Chaix, professor of geography at Geneva; Josef von Fedor, professor of hygiene at Buda Pesth, and Adolph Hirsch, professor of astronomy at Neuchatel. Two men of science lost their lives in the direct pursuit of knowledge: Dr. P. Kohlstock died at Tien-Tsin, while making researches on tropical diseases, and Dr. Menke was murdered by natives while on an exploring expedition to Macquari Island.

The erection of a memorial to Huxley in Ealing, near London, where he was born and received his early education, is contemplated. A bronze medallion portrait has been advocated for the central feature of the design, which may take the form of a simple mural tablet or of a more worthy monument as funds are obtainable. Subscriptions are not confined to the neighborhood or land of Huxley's birth, and those who may be desirous of assisting should communicate with the secretary to the fund, Mr. B. B. Woodward, 120 The Grove, Ealing, London, W.

A meeting was held at Cambridge University on April 27 to arrange for some acknowledgment of the services to science and the University of Prof. G. D. Liveing. Professor Liveing is now seventy-three years of age. In 1852 he organized the chemical laboratory at Cambridge, which was the first scientific laboratory in the University.—Mr. Herbert Spencer celebrated his eighty-first birthday on April 27. Mr. Spencer lives quietly at Brighton. His health is fair, but he is unable to undertake much literary work.—A silver loving cup was presented by a number of teachers to Mr. Thomas Meehan, the veteran horticulturist and botanist, of Philadelphia, on the occasion of his seventy-fifth birthday.

Dr. Edmund Arthur Engle, professor of mathematics at Washington University, St. Louis, and dean of the College of Engineering, has been elected president of the Worcester Polytechnic Institute.—Mr. J. H. H. Teall has succeeded Sir Archibald Geikie as director-general of the British Geological Survey.

Several important scientific positions under the government will be filled by civil service examination on June 3. These include the positions of plant physiologist and plant pathologist in the Department of Agriculture, with salaries of $1,800 per annum, and the position of ethnologist in the Bureau of American Ethnology, with a salary of $1,500. Further particulars can be obtained by addressing the Civil Service Commission at Washington, D. C.

At the last meeting of the Rumford Committee of the American Academy of Arts and Sciences a grant of $300 was awarded to Prof. Arthur A. Noyes in aid of a research on the effect of high temperatures upon the relative conductivity of aqueous salt solutions.—Dr. Edmund B. Wilson, professor of zoology at Columbia University, and Dr. J. Playfair McMurrich, professor of anatomy at the University of Michigan, are among the Americans who will attend the International Zoological Congress to be held in Berlin from the 12th to the 19th of August.

The arrangements for the celebration of the bi-centennial of the Yale University in October next have now been made public. The addresses include one by President Gilman, of the Johns Hopkins University, on 'Yale in its Relation to Science and Letters,' and one by Prof. W. H. Welch, of the same University, on 'Yale in its Relation to Medicine.'

The United States Department of Agriculture has established an agricultural experiment station in Porto Rico, which will be under the direction of Mr. Frank D. Gardner, now of the Division of Soils.

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THE TRANSMISSION OF YELLOW FEVER BY MOSQUITOES.

By GEORGE M. STERNBERG, M.D., LL.D.,
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THE discoveries which have been made during the past twenty-five years with reference to the etiology of infectious diseases constitute the greatest achievement of scientific medicine and afford a substantial basis for the application of intelligent measures of prophylaxis. We now know the specific cause ('germ') of typhoid fever, of pulmonary consumption, of cholera, of diphtheria, of erysipelas, of croupous pneumonia, of the malarial fevers and of various other infectious diseases of man and of the domestic animals, but, up to the present time, all efforts to discover the germ of yellow fever have been without success. The present writer, as a member of the Havana Yellow Fever Commission, in 1879, made the first systematic attempt to solve the unsettled questions relating to yellow fever etiology by modern methods of research. Naturally the first and most important question to engage my attention was that relating to the specific infectious agent, or 'germ,' which there was every reason to believe must be found in the bodies of infected individuals. Was this germ present in the blood, as in the case of relapsing fever; or was it to be found in the organs and tissues which upon post mortem examination give evidence of pathological changes, as in typhoid fever, pneumonia and diphtheria; or was it to be found in the alimentary canal, as in cholera and dysentery. The clinical history of the disease indicated a general blood infection. As my equipment included the best microscopical apparatus made, I had strong hopes that in properly stained preparations of blood taken from the circulation of yellow fever patients my Zeiss 1-18 oil immersion objective would reveal to me the germ I was in search of. But I was doomed to disap-

pointment. Repeated examinations of blood from patients in every stage of the disease failed to demonstrate the presence of micro-organisms of any kind. My subsequent investigations in Havana, Vera Cruz and Rio de Janeiro, made in 1887, 1888 and 1889, were equally unsuccessful. And numerous competent microscopists of various nations have since searched in vain for this elusive germ. Another method of attacking this problem consists in introducing blood from yellow fever patients or recent cadavers into various 'culture media' for the purpose of cultivating any germ that might be present. Extended researches of this kind also gave a negative result, which in my final report I stated as follows:

The specific cause of yellow fever has not yet been demonstrated.

It is demonstrated that micro-organisms, capable of development in the culture-media usually employed by bacteriologists, are only found in the blood and tissues of yellow fever cadavers in exceptional cases, when cultures are made very soon after death.

Since this report was made various investigators have attacked the question of yellow fever etiology, and one of them has made very positive claims to the discovery of the specific germ. I refer to the Italian bacteriologist, Sanarelli. His researches were made in Brazil, and, singularly enough, he found in the blood of the first case examined by him a bacillus. It was present in large numbers, but this case proved to be unique, for neither Sanarelli nor any one else has since found it in such abundance. It has been found in small numbers in the blood and tissues of yellow fever cadavers in a certain number of the cases examined. But carefully conducted researches by competent bacteriologists have failed to demonstrate its presence in a considerable proportion of the cases, and the recent researches of Reed, Carroll and Agramonte, to which I shall shortly refer, demonstrate conclusively that the bacillus of Sanarelli has nothing to do with the etiology of yellow fever.

So far as I am aware, Dr. Carlos Finlay, of Havana, Cuba, was the first to suggest the transmission of yellow fever by mosquitoes. In a communication made to the Academy of Sciences of Havana, in October, 1881, he gave an account of his first attempts to demonstrate the truth of his theory. In a paper contributed to the 'Edinburgh Medical Journal' in 1894 Dr. Finlay gives a summary of his experimental inoculations up to that date as follows:

A summary account of the experiments performed by myself (and some also by my friend, Dr. Delgado), during the last twelve years, will enable the reader to judge for himself. The experiment has consisted in first applying a captive mosquito to a yellow fever patient, allowing it to introduce its lance and to fill itself with blood; next, after the lapse of two or more days, applying the same mosquito to the skin of a person who is considered susceptible to yellow fever; and, finally, observing the effects, not only during the first few weeks, but during periods of several years, so as to appreciate the amount of immunity that should follow.

Between the 30th of June, 1881, and the 2d of December, 1893, eighty-eight persons have been so inoculated. All were white adults, uniting the conditions which justify the assumption that they were susceptible to yellow fever. Only three were women. The chronological distribution of the inoculations was as follows: Seven in 1881, ten in 1883, nine in 1885, three in 1886, twelve in 1887, nine in 1888, seven in 1889, ten in 1890, eight in 1891, three in 1892, and ten in 1893.

The yellow fever patients upon whom the mosquitoes were contaminated were, almost in every instance, well-marked cases of the albuminuric or melano-albuminuric forms, in the second, third, fourth, fifth, or sixth day of the disease. In some of the susceptible subjects, the inoculation was repeated when the source of the contamination appeared uncertain.

Among the eighty-seven who have been under observation, the following results have been recorded:

Within a term of days, varying between five and twenty-five after the inoculation, *one* presented a mild albuminuric attack, and *thirteen only* 'acclimation fevers.'

While Finlay's theory appeared to be plausible and to explain many of the facts relating to the etiology of yellow fever, his experimental inoculations not only failed to give it substantial support, but the negative results, as reported by himself, seemed to be opposed to the view that yellow fever is transmitted by the mosquito. It is true that he reports one case which 'presented a mild albuminuric attack' which we may accept as an attack of yellow fever. But in view of the fact that this case occurred in the city of Havana, where yellow fever is endemic, and of the eighty-six negative results from similar inoculations, the inference seemed justified that in this case the disease was contracted in some other way than as a result of the so-called 'mosquito inoculation.' The thirteen cases in which 'only acclimation fevers' occurred 'within a term of days varying between five and twenty-five after the inoculation' appeared to me to have no value as giving support to Finlay's theory; first, because these 'acclimation fevers' could not be identified as mild cases of yellow fever; second, because the ordinary period of incubation in yellow fever, is less than five days; and, third, because these individuals, having recently arrived in Havana, were liable to attacks of yellow fever, or of 'acclimation fever' as a result of their residence in this city and quite independently of Dr. Finlay's mosquito inoculations. For these reasons Dr. Finlay's experiments failed to convince the medical profession generally of the truth of his theory relating to the transmission of yellow fever, and this important question remained in doubt and a subject of controversy. One party regarded the disease as personally contagious and supposed it to be communicated directly from the sick to the well, as in the case of other contagious diseases, such as smallpox, scarlet fever, etc. Opposed to this theory was the fact that in innumerable instances non-immune persons had been known to care for yellow fever patients as nurses, or physicians, without contracting the

disease; also the fact that the epidemic extension of the disease depends upon external conditions relating to temperature, altitude, rainfall, etc. It was a well-established fact that the disease is arrested by cold weather and does not prevail in northern latitudes or at considerable altitudes, But diseases which are directly transmitted from man to man by personal contact have no such limitations. The alternate theory took account of the above-mentioned facts and assumed that the disease was indirectly transmitted from the sick to the well, as is the case in typhoid fever and cholera, and that its germ was capable of development external to the human body when conditions were favorable. These conditions were believed to be a certain elevation of temperature, the presence of moisture and suitable organic pabulum (filth) for the development of the germ. The two first-mentioned conditions were known to be essential, the third was a subject of controversy.

Yellow fever epidemics do not occur in the winter months in the temperate zone and they do not occur in arid regions. As epidemics have frequently prevailed in sea-coast cities known to be in an insanitary condition, it has been generally assumed that the presence of decomposing organic material is favorable for the development of an epidemic and that, like typhoid fever and cholera, yellow fever is a 'filth disease.' Opposed to this view, however, is the fact that epidemics have frequently occurred in localities (*e. g.*, at military posts) where no local insanitary conditions were to be found. Moreover, there are marked differences in regard to the transmission of the recognized filth diseases —typhoid fever and cholera—and yellow fever. The first-mentioned diseases are largely propagated by means of a contaminated water supply, whereas there is no evidence that yellow fever is ever communicated in this way. Typhoid fever and cholera prevail in all parts of the world and may prevail at any season of the year, although cholera, as a rule, is a disease of the summer months. On the other hand, yellow fever has a very restricted area of prevalence and is essentially a disease of seaboard cities and of warm climates. Evidently neither of the theories referred to accounts for all of the observed facts with reference to the endemic prevalence and epidemic extension of the disease under consideration.

Having for years given much thought to this subject, I became some time since impressed with the view that probably in yellow fever, as in the malarial fevers, there is an 'intermediate host.' I therefore suggested to Dr. Reed, president of the board* appointed upon my recommendation for the study of this disease in the Island of Cuba, that he should give special attention to the possibility of transmission by some

* The members of the board were: Major Walter Reed, Surgeon U. S. A.; Dr. James Carroll, Contract Surgeon U. S. A.; Dr. A. Agramonte, Contract Surgeon U. S. A., and Dr. Jesse W. Lazear, Contract Surgeon U. S. A.

insect, although the experiments of Finlay seemed to show that this insect was not a mosquito of the genus *Culex*, such as he had used in his inoculation experiments. I also urged that efforts should be made to ascertain definitely whether the disease can be communicated from man to man by blood inoculations. Evidently if this is the case the blood must contain the living infectious agent upon which the propagation of the disease depends, notwithstanding the fact that all attempts to demonstrate the presence of such a germ in the blood, by means of the microscope and culture methods, had proved unavailing. I had previously demonstrated by repeated experiments that inoculations of yellow fever blood into lower animals—dogs, rabbits, guinea-pigs—give a negative result, but this negative result might well be because these animals were not susceptible to the disease and could not be accepted as showing that the germ of yellow fever was not present in the blood. A single inoculation experiment on man had been made in my presence in the city of Vera Cruz, in 1887, by Dr. Daniel Ruiz, who was in charge of the civil hospital in that city. But this experiment was inconclusive for the reason that the patient from whom the blood was obtained was in the eighth day of the disease, and it was quite possible that the specific germ might have been present at an earlier period and that after a certain number of days the natural resources of the body are sufficient to effect its destruction, or in some way to cause its disappearance from the circulation.

This was the status of the question of yellow fever etiology when Dr. Reed and his associates commenced their investigations in Cuba during the summer of 1900. In a 'Preliminary Note,' read at the meeting of the American Public Health Association, October 22, 1900, the board gave a report of three cases of yellow fever which they believed to be the direct result of mosquito inoculations. Two of these were members of the board, viz.: Dr. Jesse W. Lazear and Dr. James Carroll, who voluntarily submitted themselves to the experiment. Dr. Carroll suffered a severe attack of the disease and recovered, but Dr. Lazear fell a victim to his enthusiasm in the cause of science and humanity. His death occurred on September 25th, after an illness of six days' duration. About the same time nine other individuals who volunteered for the experiment were bitten by infected mosquitoes—*i. e.,* by mosquitoes which had previously been allowed to fill themselves with blood from yellow fever cases—and in these cases the result was negative. In considering the experimental evidence thus far obtained the attention of the members of the board was attracted by the fact that in the nine inoculations with a negative result "the time elapsing between the biting of the mosquito and the inoculation of the healthy subject varied in seven cases from two to eight days and in the remaining two from ten to thirteen days, whereas in two of the three successful cases the mosquito had been kept

for twelve days or longer." In the third case, that of Dr. Lazear, the facts are stated in the report of the board as follows:

Case 3. Dr. Jesse W. Lazear, Acting Assistant Surgeon U. S. Army, a member of this board, was bitten on August 16, 1900 (Case 3, Table III) by a mosquito *(Culex fasciatus)* which ten days previously had been contaminated by biting a very mild case of yellow fever (fifth day). No appreciable disturbance of health followed this inoculation.

On September 13, 1900 (forenoon), Dr. Lazear, while on a visit to Las Animas Hospital, and while collecting blood from yellow fever patients for study, was bitten by a Culex mosquito (variety undetermined). As Dr. Lazear had been previously bitten by a contaminated insect without after effects, he deliberately allowed this particular mosquito, which had settled on the back of his hand, to remain until it had satisfied its hunger.

On the evening of September 18, 5 days after the bite, Dr. Lazear complained of feeling 'out of sorts,' and had a chill at 8 p. m.

On September 19, 12 o'clock noon, his temperature was 102.4°, pulse 112; his eyes were injected and his face suffused; at 3 p. m. temperature was 103.4°, pulse 104; 6 p. m., temperature 103.8° and pulse 106; albumin appeared in the urine. Jaundice appeared on the third day. The subsequent history of this case was one of progressive and fatal yellow fever, the death of our much-lamented colleague having occurred on the evening of September 25, 1900.

Evidently in this case the evidence is not satisfactory as to the fatal attack being a result of the bite by a mosquito 'while on a visit to Las Animas Hospital,' although Dr. Lazear himself was thoroughly convinced that this was the direct cause of his attack.

The inference drawn by Dr. Reed and his associates, from the experiments thus far made, was that yellow fever may be transmitted by mosquitoes of the genus *Culex,* but that in order to convey the infection to a non-immune individual the insect must be kept for 12 days or longer after it has filled itself with blood from a yellow fever patient in the earlier stages of the disease. In other words, that a certain period of incubation is required in the body of the insect before the germ reaches its salivary glands and consequently before it is able to inoculate an individual with the germs of yellow fever. This inference, based upon experimental data, received support from other observations, which have been repeatedly made, with reference to the introduction and spread of yellow fever in localities favorable to its propagation. When a case is imported to one of our southern seaport cities, from Havana, Vera Cruz or some other endemic focus of the disease, an interval of two weeks or more occurs before secondary cases are developed as a result of such importation. In the light of our present knowledge this is readily understood. A certain number of mosquitoes having filled themselves with blood from this first case after an interval of twelve days or more bite non-immune individuals living in the vicinity, and these individuals after a brief period of incubation fall sick with the disease; being bitten by other mosquitoes they serve to transmit the disease

through the 'intermediate host' to still others. Thus the epidemic extends, at first slowly as from house to house, then more rapidly, as by geometrical progression.

It will be seen that the essential difference between the successful experiments of the board of which Dr. Reed is president and the unsuccessful experiments of Finlay consists of the length of time during which the mosquitoes were kept after filling themselves with blood from a yellow-fever patient. In Finlay's experiments the interval was usually short—from two to five or six days, and it will be noted that in the experiments of Reed and his associates the result was invariably negative when the insect had been kept for less than eight days (7 cases).

Having obtained what they considered satisfactory evidence that yellow fever is transmitted by mosquitoes, Dr. Reed and his associates proceeded to extend their experiments for the purpose of establishing the fact in such a positive manner that the medical profession and the scientific world generally might be convinced of the reliability of the experimental evidence upon which their conclusions were based. These conclusions, which have been fully justified by their subsequent experiments were stated in their 'Preliminary Note' as follows:

1. Bacillus icteroides (Sanarelli) stands in no causative relation to yellow fever, but, when present, should be considered as a secondary invader in this disease.

2. The mosquito serves as the intermediate host for the parasite of yellow fever.

In 'An Additional Note' read at the Pan-American Medical Congress held in Havana, Cuba, February 4-7, 1901, a report is made of the further experiments made up to that date. In order that the absolute scientific value of these experiments may be fully appreciated I shall quote quite freely from this report with reference to the methods adopted for the purpose of excluding all sources of infection other than the mosquito inoculation:

In order to exercise perfect control over the movements of those individuals who were to be subjected to experimentation, and to avoid any other possible source of infection, a location was selected in an open and uncultivated field, about one mile from the town of Quemados, Cuba. Here an experimental sanitary station was established under the complete control of the senior member of this Board. This station was named Camp Lazear, in honor of our late colleague, Dr. Jesse W. Lazear, Acting Assistant Surgeon U. S. A., who died of yellow fever, while courageously investigating the causation of this disease. The site selected was well drained, freely exposed to sunlight and winds, and from every point of view satisfactory for the purposes intended.

The personnel of this camp consisted of two medical officers, Dr. Roger P. Ames, Acting Assistant Surgeon U. S. A., an immune, in immediate charge; Dr. R. P. Cooke, Acting Assistant Surgeon U. S. A., non-immune; one acting hospital steward, an immune; nine privates of the hospital corps, one of whom was immune, and one immune ambulance driver.

For the quartering of this detachment, and of such non-immune individuals as should be received for experimentation, hospital tents, properly floored, were provided. These were placed at a distance of about twenty feet from each other, and were numbered 1 to 7 respectively.

Camp Lazear was established Nov. 20, 1900, and from this date was strictly quarantined, no one being permitted to leave or enter camp except the three immune members of the detachment and the members of the board. Supplies were drawn chiefly from Columbia Barracks, and for this purpose a conveyance under the control of an immune acting hospital steward, and having an immune driver, was used.

A few Spanish immigrants recently arrived at the port of Havana were received at Camp Lazear, from time to time, while these observations were being carried out. A non-immune person, having once left this camp, was not permitted to return to it under any circumstances whatever.

The temperature and pulse of all non-immune residents were carefully recorded three time a day. Under these circumstances any infected individual entering the camp could be promptly detected and removed. As a matter of fact, only two persons, not the subject of experimentation, developed any rise of temperature; one, a Spanish immigrant, with probable commencing pulmonary tuberculosis, who was discharged at the end of three days; and the other, a Spanish immigrant, who developed a temperature of 102.6° F. on the afternoon of his fourth day in camp. He was at once removed with his entire bedding and baggage and placed in the receiving ward at Columbia Barracks. His fever, which was marked by daily intermissions for three days, subsided upon the administration of cathartics and enemata. His attack was considered to be due to intestinal irritation. He was not permitted, however, to return to the camp.

No non-immune resident was subjected to inoculation who had not passed in this camp the full period of incubation of yellow fever, with one exception, to be hereinafter mentioned.

For the purpose of experimentation subjects were selected as follows: From Tent No. 2, 2 non-immunes, and from Tent No. 5, 3 non-immunes. Later, 1 non-immune in Tent No. 6 was also designated for inoculation.

It should be borne in mind that at the time when these inoculations were begun, there were only 12 non-immune residents at Camp Lazear, and that 5 of these were selected for experiment, viz., 2 in Tent No. 2, and 3 in Tent No. 5. Of these we succeeded in infecting 4, viz., 1 in Tent No. 2 and 3 in Tent No. 5, each of whom developed an attack of yellow fever within the period of incubation of this disease. The one negative result, therefore, was in Case 2—Moran—inoculated with a mosquito on the fifteenth day after the insect had bitten a case of yellow fever on the third day. Since this mosquito failed to infect Case 4, three days after it had bitten Moran, it follows that the result could not have been otherwise than negative in the latter case. We now know, as the result of our observations, that in the case of an insect kept at room temperature during the cool weather of November, fifteen or even eighteen days would, in all probability, be too short a time to render it capable of producing the disease.

As bearing upon the source of infection, we invite attention to the period of time during which the subjects had been kept under rigid quarantine, prior to successful inoculation, which was as follows: Case 1, fifteen days; Case 3, nine days; Case 4, nineteen days; Case 5, twenty-one days. We further desire to emphasize the fact that this epidemic of yellow fever, which affected 33.33 per cent. of the non-immune residents of Camp Lazear, did not concern the 7 non-immunes occupying Tents Nos. 1, 4, 6 and 7, *but was strictly limited to those individuals who had been bitten by contaminated mosquitoes.*

Nothing could point more forcibly to the source of this infection than the order of the occurrence of events at this camp. The precision with which the infection of the individual followed the bite of the mosquito left nothing to be desired in order to fulfill the requirements of a scientific experiment.

In summing up their results at the conclusion of this report the following statement is made:

Out of a total of 18 non-immunes whom we have inoculated with contaminated mosquitoes, since we began this line of investigation, 8, or 44.4 per cent., have contracted yellow fever. If we exclude those individuals bitten by mosquitoes that had been kept less than twelve days after contamination, and which were therefore probably incapable of conveying the disease, we have to record eight positive and two negative results—80 per cent.

In a still later report (May, 1901) Dr. Reed says: "We have thus far succeeded in conveying yellow fever to twelve individuals by means of the bites of contaminated mosquitoes."

The non-immune individuals experimented upon were all fully informed as to the nature of the experiment and its probable results and all gave their full consent. Fortunately no one of these brave volunteers in the cause of science and humanity suffered a fatal attack of the disease, although several were very ill and gave great anxiety to the members of the board, who fully appreciated the grave responsibility which rested upon them. That these experiments were justifiable under the circumstances mentioned is, I believe, beyond question. In no other way could the fact established have been demonstrated and the knowledge gained is of inestimable value as a guide to reliable measures of prevention. Already it is being applied in Cuba and without doubt innumerable lives will be saved as a result of these experiments showing the precise method by which yellow fever is contracted by those exposed in an 'infected locality.' Some of these volunteers were enlisted men of the United States Army and some were Spanish immigrants who had recently arrived in Cuba. When taken sick they received the best possible care and after their recovery they had the advantage of being 'immunes' who had nothing further to fear from the disease which has caused the death of thousands and tens of thousands of Spanish soldiers and immigrants who have come to Cuba under the orders of their Government or to seek their fortunes.

The experiments already referred to show in the most conclusive manner that the blood of yellow-fever patients contains the infectious agent, or germ, to which the disease is due, and this has been further demonstrated by direct inoculations from man to man. This experiment was made by Dr. Reed at 'Camp Lazear' upon four individuals, who freely consented to it; and in three of the four a typical attack of yellow fever resulted from the blood injection. The blood was taken from a vein at the bend of the elbow on the first or second day of sickness and was injected subcutaneously into the four non-immune in-

dividuals, the amount being in one positive case 2 c. c., in one 1.5 c. c., and in one 0.5 c. c. In the case attended with a negative result, a Spanish immigrant, a mosquito inoculation also proved to be without effect, and Dr. Reed supposes that this individual 'probably possesses a natural immunity to yellow fever.' Dr. Reed says with reference to these experiments:

> It is important to note that in the three cases in which the injection of the blood brought about an attack of yellow fever, careful culture from the same blood, taken immediately after injection, failed to show the presence of Sanarelli's bacillus.

Having demonstrated the fact that yellow fever is propagated by mosquitoes Dr. Reed and his associates have endeavored to ascertain whether it may also be propagated, as has been commonly supposed, by clothing, bedding and other articles which have been in use by those sick with this disease. With reference to the experiments made for the solution of this question I cannot do better than to quote *in extenso* from Dr. Reed's paper read at the Pan-American Medical Congress in Havana. He says:

> We believe that the general consensus of opinion both of the medical profession and of the laity is strongly in favor of the conveyance of yellow fever by fomites. The origin of epidemics, devastating in their course, has been frequently attributed to the unpacking of trunks and boxes that contained supposedly infected clothing; and hence the efforts of health authorities, both state and national, are being constantly directed to the thorough disinfection of all clothing and bedding shipped from ports where yellow fever prevails. To such extremes have efforts at disinfection been carried, in order to prevent the importation of this disease into the United States, that, during the epidemic season, all articles of personal apparel and bedding have been subjected to disinfection, sometimes both at the port of departure and at the port of arrival; and this has been done whether the articles have previously been contaminated by contact with yellow-fever patients or not. The mere fact that the individual has resided, even for a day, in a city where yellow fever is present, has been sufficient cause to subject his baggage to rigid disinfection by the sanitary authorities.
>
> To determine, therefore, whether clothing and bedding which have been contaminated by contact with yellow fever patients and their discharges can convey this disease is a matter of the utmost importance. Although the literature contains many references to the failure of such contaminated articles to cause the disease, we have considered it advisable to test, by actual experiment on non-immune human beings, the theory of the conveyance of yellow fever by fomites, since we know of no other way in which this question can ever be finally determined.
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> For this purpose there was erected at Camp Lazear a small frame house consisting of one room 14 x 20 feet and known as 'Building No. 1,' or the 'Infected Clothing and Bedding Building.' The cubic capacity of this house was 2,800 feet. It was tightly ceiled within with 'tongue-and-grooved' boards, and was well battened on the outside. It faced to the south and was provided with two small windows, each 26 x 34 inches in size. These windows were both placed on the south side of the building, the purpose being to prevent, as much as pos-

sible, any thorough circulation of the air within the house. They were closed by permanent wire screens of .5 mm. mesh. In addition sliding glass sash were provided within and heavy wooden shutters without; the latter intended to prevent the entrance of sunlight into the building, as it was not deemed desirable that the disinfecting qualities of sunlight, direct or diffused, should at any time be exerted on the articles of clothing contained within this room. Entrance was effected through a small vestibule, 3 x 5 feet, also placed on the southern side of the house. This vestibule was protected without by a solid door and was divided in its middle by a wire screen door, swung on spring hinges. The inner entrance was also closed by a second wire screen door. In this way the passage of mosquitoes into this room was effectually excluded. During the day, and until after sunset, the house was kept securely closed, while by means of a suitable heating apparatus the temperature was raised to 92° to 95° F. Precaution was taken at the same time to maintain a sufficient humidity of the atmosphere. The average temperature of this house was thus kept at 76.2° F. for a period of sixty-three days.

Nov. 30, 1900, the building now being ready for occupancy, three large boxes filled with sheets, pillow-slips, blankets, etc., contaminated by contact with cases of yellow fever and their discharges were received and placed therein. The majority of the articles had been taken from the beds of patients sick with yellow fever at Las Animas Hospital, Havana, or at Columbia Barracks. Many of them had been purposely soiled with a liberal quantity of black vomit, urine, and fecal matter. A dirty 'comfortable' and a much-soiled pair of blankets, removed from the bed of a patient sick with yellow fever in the town of Quemados, were contained in one of these boxes. The same day, at 6 p. m., Dr. R. P. Cooke, Acting Assistant-Surgeon U. S. A., and two privates of the hospital corps, all non-immune young Americans, entered this building and deliberately unpacked these boxes, which had been tightly closed and locked for a period of two weeks. They were careful at the same time to give each article a thorough handling and shaking, in order to disseminate through the air of the room the specific agent of yellow fever, if contained in these fomites. These soiled sheets, pillow-cases, and blankets were used in preparing the beds in which the members of the hospital corps slept. Various soiled articles were hung around the room and placed about the bed occupied by Dr. Cooke.

From this date until Dec. 19, 1900, a period of twenty days, this room was occupied each night by these three non-immunes. Each morning the various soiled articles were carefully packed in the aforesaid boxes, and at night again unpacked and distributed about the room. During the day the residents of this house were permitted to occupy a tent pitched in the immediate vicinity, but were kept in strict quarantine.

December 12, a fourth box of clothing and bedding was received from Las Animas Hospital. These articles had been used on the beds of yellow-fever patients, but in addition had been purposely soiled with the bloody stools of a fatal case of this disease. As this box had been packed for a number of days, when opened and unpacked by Dr. Cooke and his assistants, on December 12th, the odor was so offensive as to compel them to retreat from the house. They pluckily returned, however, within a short time and spent the night as usual.

December 19 these three non-immunes were placed in quarantine for five days and then given the liberty of the camp. All had remained in perfect health, notwithstanding their stay of twenty nights amid such unwholesome surroundings.

During the week, December 20-27, the following articles were also placed in this house, viz.: pajamas suits, 1; undershirts, 2; night-shirts, 4; pillow-slips,

4; sheets, 6; blankets, 5; pillows, 2; mattresses, 1. These articles had been removed from the persons and beds of four patients sick with yellow fever and were very much soiled, as any change of clothing or bed-linen during their attacks had been purposely avoided, the object being to obtain articles as thoroughly contaminated as possible.

From Dec. 21, 1900, till Jan. 10, 1901, this building was again occupied by two non-immune young Americans, under the same conditions as the preceding occupants, except that these men slept every night in the very garments worn by yellow fever patients throughout their entire attacks, besides making use exclusively of their much-soiled pillow-slips, sheets, and blankets. At the end of twenty-one nights of such intimate contact with these fomites, they also went into quarantine, from which they were released five days later in perfect health.

From January 11 till January 31, a period of twenty days, 'Building No. 1' continued to be occupied by two other non-immune Americans, who, like those who preceded them, have slept every night in the beds formerly occupied by yellow fever patients and in the night-shirts used by these patients throughout the attack, without change. In addition, during the last fourteen nights of their occupancy of this house they have slept, each night, with their pillows covered with towels that had been thoroughly soiled with the blood drawn from both the general and capillary circulation, on the first day of the disease, in the case of a well-marked attack of yellow fever. Notwithstanding this trying ordeal, these men have continued to remain in perfect health.

The attempt which we have therefore made to infect 'Building No. 1,' and its seven non-immune occupants, during a period of sixty-three days, has proved an absolute failure. We think we cannot do better here than to quote from the classic work of La Roche.* This author says: "In relation to the yellow fever, we find so many instances establishing the fact of the non-transmissibility of the disease through the agency of articles of the kind mentioned, and of merchandise generally, that we cannot but discredit the accounts of a contrary character assigned in medical writings, and still more to those presented on the strength of popular report solely. For if, in a large number of well-authenticated cases, such articles have been handled and used with perfect impunity—and that, too, often under circumstances best calculated to insure the effect in question—we have every reason to conclude that a contrary result will not be obtained in other instances of a similar kind; and that consequently the effect said to have been produced by exposure to those articles, must—unless established beyond the possibility of doubt—be referred to some other agency."

The question here naturally arises: How does a house become infected with yellow fever? This we have attempted to solve by the erection at Camp Lazear of a second house, known as 'Building No. 2,' or the 'Infected Mosquito Building.' This was in all respects similar to 'Building No. 1,' except that the door and windows were placed on opposite sides of the building so as to give through-and-through ventilation. It was divided, also, by a wire-screen partition, extending from floor to ceiling, into two rooms, 12 x 14 feet and 8 x 14 feet respectively. Whereas, all articles admitted to 'Building No. 1' had been soiled by contact with yellow fever patients, all articles admitted to 'Building No. 2' were first carefully disinfected by steam before being placed therein.

On Dec. 21, 1900, at 11.45 a. m., there were set free in the larger room of this building fifteen mosquitoes—*C. fasciatus*—which had previously been contaminated by biting yellow fever patients, as follows: 1, a severe case, on the second day, Nov. 27, 1900, twenty-four days; 3, a well-marked case, on the first

* R. La Roche: Yellow Fever, Vol. II, p. 516, Philadelphia.

day, Dec. 9, 1900, twelve days; 4, a mild case, on the first day, Dec. 13, 1900, eight days; 7, a well-marked case, on the first day, Dec. 16, 1900, five days—total, 15.

Only one of these insects was considered capable of conveying the infection, viz., the mosquito that had bitten a severe case twenty-four days before; while three others—the twelve-day insects—had possibly reached the dangerous stage, as they had been kept at an average temperature of 82° F.

At 12, noon, of the same day, John J. Moran—already referred to as Case 2 in this report—a non-immune American, entered the room where the mosquitoes had been freed, and remained thirty minutes. During this time he was bitten about the face and hands by several insects. At 4.30 p. m., the same day, he again entered and remained twenty minutes, and was again bitten. The following day, at 4.30 p. m., he, for the third time, entered the room, and was again bitten.

Case 7.—On Dec. 25, 1900, at 6 a. m., the fourth day, Moran complained of slight dizziness and frontal headache. At 11 a. m. he went to bed, complaining of increased headache and malaise, with a temperature of 99.6° F., pulse 88; at noon the temperature was 100.4° F., the pulse 98; at 1 p. m., 101.2° F., the pulse 96, and his eyes were much injected and face suffused. He was removed to the yellow fever wards. He was seen on several occasions by the board of experts and the diagnosis of yellow fever confirmed.

The period of incubation in this case, dating from the first visit to 'Building No. 2,' was three days and twenty-three hours. If reckoned from his last visit it was two days and eighteen hours. There was no other possible source for his infection, as he had been strictly quarantined at Camp Lazear for a period of thirty-two days prior to his exposure in the mosquito building.

During each of Moran's visits, two non-immunes remained in this same building, only protected by the wire-screen partition. From Dec. 21, 1900, till Jan. 8, 1901, inclusive—eighteen nights—these non-immunes have slept in this house, only protected by the wire-screen partition. These men have remained in perfect health to the present time.

Thus at Camp Lazear, of 7 non-immunes whom we attempted to infect by means of the bites of contaminated mosquitoes, we have succeeded in conveying the disease to 6, or 85.71 per cent. On the other hand, of 7 non-immunes whom we tried to infect by means of fomites, under particularly favorable circumstances, we did not succeed in a single instance.

It is evident that in view of our present knowledge relating to the mode of transmission of yellow fever, the preventive measures which have heretofore been considered most important, *i. e.*, isolation of the sick, disinfection of clothing and bedding, and municipal sanitation—are either of no avail or of comparatively little value. It is true that yellow fever epidemics have resulted, as a rule, from the introduction to a previously healthy locality of one or more persons suffering from the disease. But we now know that its extension did not depend upon the direct contact of the sick with the non-immune individuals and that isolation of the sick from such contact is unnecessary and without avail. On the other hand complete isolation from the agent which is responsible for the propagation of the disease is all-important. In the absence of a yellow-fever patient from which to draw blood the mosquito is harmless, and in the absence of the mosquito the yellow-fever patient is

harmless—as the experimental evidence now stands. Yellow fever epidemics are terminated by cold weather because then the mosquitoes die or become torpid. The sanitary condition of our southern seaport cities is no better in winter than in summer and if the infection attached to clothing and bedding it is difficult to understand why the first frosts of autumn should arrest the progress of an epidemic. But all this is explained now that the mode of transmission has been demonstrated.

Insanitary local conditions may, however, have a certain influence in the propagation of the disease, for it has been ascertained that the species of mosquito which serves as an intermediate host for the yellow fever germ may breed in cesspools and sewers, as well as in stagnant pools of water. If, therefore, the streets of a city are unpaved and ungraded and there are open spaces where water may accumulate in pools, as well as open cesspools to serve as breeding places for *Culex fasciatus,* that city will present conditions more favorable for the propagation of yellow fever than it would if well paved and drained and sewered.

The question whether yellow fever may be transmitted by any other species of mosquito than *Culex fasciatus* has not been determined. Facts relating to the propagation of the disease indicate that the mosquito which serves as an intermediate host for the yellow-fever germ has a somewhat restricted geographical range and is to be found especially upon the sea-coast and the margins of rivers in the so-called 'yellow fever zone.' While occasional epidemics have occurred upon the southwest coast of the Iberian peninsula, the disease, as an epidemic, is unknown elsewhere in Europe, and there is no evidence that it has ever invaded the great and populous continent of Asia. In Africa it is limited to the west coast. In North America, although it has occasionally prevailed as an epidemic in every one of our seaport cities as far north as Boston, and in the Mississippi Valley as far north as St. Louis, it has never established itself as an endemic disease within the limits of the United States. Vera Cruz, and probably other points on the Gulf coast of Mexico, are, however, at the present time endemic foci of the disease. In South America it has prevailed as an epidemic at all of the seaports on the Gulf and Atlantic Coasts, as far south as Montevideo and Buenos Ayres, and on the Pacific along the coast of Peru.

The region in which the disease has had the greatest and most frequent prevalence is bounded by the shores of the Gulf of Mexico, and includes the West India islands. Within the past few years yellow fever has been carried to the west coast of North American, and has prevailed as an epidemic as far north as the Mexican port of Guaymas, on the Gulf of California.

It must not be supposed that *Culex fasciatus* is only found where

yellow fever prevails. The propagation of the disease depends upon the introduction of an infected individual to a locality where this mosquito is found, at a season of the year when it is active. Owing to the short period of incubation (five days or less), the brief duration of the disease and especially of the period during which the infectious agent (germ) is found in the blood, it is evident that ships sailing from infected ports, upon which cases of yellow fever develop, are not likely to introduce the disease to distant seaports. The continuance of an epidemic on ship-board, as on the land, must depend upon the presence of infected mosquitoes and of non-immune individuals. Under these conditions we can readily understand why the disease should not be carried from the West Indies or from South America to the Mediterranean, to the east coast of Africa or to Asiatic seaport cities. On the other hand, if the disease could be transmitted by infected clothing, bedding, etc., there seems no good reason why it should not have been carried to these distant localities long ago.

The restriction as regards altitude, however, probably depends upon the fact that the mosquito which serves as an intermediate host is a coast species, which does not live in elevated regions. It is a well-established fact that yellow fever has never prevailed in the City of Mexico, although this city has constant and unrestricted intercourse with the infected seaport, Vera Cruz. Persons who have been exposed in Vera Cruz during the epidemic season frequently fall sick after their arrival in the City of Mexico, but they do not communicate the disease to those in attendance upon them or to others in the vicinity. Evidently some factor essential for the propagation of the disease is absent, although we have the sick man, his clothing and bedding and the insanitary local conditions which have been supposed to constitute an essential factor. I am not aware that any observations have been made with reference to the presence or absence of *Culex fasciatus* in high altitudes, but the inference that it is not to be found in such localities as the City of Mexico seems justified by the established facts already referred to.

As pointed out by Hirsch, "the disease stops short at many points in the West Indies where the climate is still in the highest degree tropical." In the Antilles it has rarely appeared at a height of more than 700 feet. In the United States the most elevated locality in which the disease has prevailed as an epidemic is Chattanooga, Tenn., which is 745 feet above sea level.

It will be remembered that the malarial fevers are contracted as a result of inoculation by mosquitoes of the genus *Anopheles,* and that the malarial parasite has been demonstrated not only in the blood of those suffering from malarial infection, but also in the stomach and salivary glands of the mosquito. If the yellow fever parasite resembled that of the malarial fevers it would no doubt have been discovered long

ago. But, as a matter of fact, this parasite, which we now know is present in the blood of those sick with the disease, has thus far eluded all researches. Possibly it is ultra-microscopic. However this may be, it is not the only infectious disease germ which remains to be discovered. There is without doubt a living germ in vaccine lymph and in the virus from smallpox pustules, but it has not been demonstrated by the microscope. The same is true of foot and mouth disease and of infectious pleuro-pneumonia of cattle, although we know that a living element of some kind is present in the infectious material by which these diseases are propagated. In Texas fever, of cattle, which is transmitted by infected ticks, the parasite is very minute, but by proper staining methods and a good microscope it may be detected in the interior of the red blood corpuscles. Drs. Reed and Carroll are at present engaged in a search for the yellow fever germ in the blood and in the bodies of infected mosquitoes. What success may attend their efforts remains to be seen, but at all events the fundamental facts have been demonstrated that this germ is present in the blood and that the disease is transmitted by a certain species of mosquito—*C. fasciatus*.

The proper measures of prophylaxis in view of this demonstration are given in the following circular, which was submitted for my approval by the Chief Surgeon, Department of Cuba, and has recently been published by the Commanding General of that Department, who, until quite recently, was a member of the Medical Corps of the Army:

CIRCULAR, No. 5.

HEADQUARTERS DEPARTMENT OF CUBA, *Havana, April 27, 1901.*

Upon the recommendation of the Chief Surgeon of the Department, the following instructions are published and will be strictly enforced at all military posts in this Department:

The recent experiments made in Havana by the Medical Department of the Army having proved that yellow fever, like malarial fever, is conveyed chiefly, and probably exclusively, by the bite of infected mosquitoes, important changes in the measures used for the prevention and treatment of this disease have become necessary.

1. In order to prevent the breeding of mosquitoes and protect officers and men against their bites, the provisions of General Orders No. 6, Department of Cuba, December 21, 1900, shall be carefully carried out, especially during the summer and fall.

2. So far as yellow fever is concerned, infection of a room or building simply means that it contains infected mosquitoes, that is, mosquitoes which have fed on yellow-fever patients. Disinfection, therefore, means the employment of measures aimed at the destruction of these mosquitoes. The most effective of these measures is fumigation, either with sulphur, formaldehyde or insect powder. The fumes of sulphur are the quickest and most effective insecticide but are otherwise objectionable. Formaldehyde gas is quite effective if the infected rooms are kept closed and sealed for two or three hours. The smoke of insect powder has also been proved very useful; it readily stupefies mosquitoes, which drop to the floor and can then be easily destroyed.

The washing of walls, floors, ceilings and furniture with disinfectants is unnecessary.

3. As it has been demonstrated that yellow fever cannot be conveyed by fomites, such as bedding, clothing, effects and baggage, they need not be subjected to any special disinfection. Care should be taken, however, not to remove them from the infected rooms until after formaldehyde fumigation, so that they may not harbor infected mosquitoes.

Medical officers taking care of yellow-fever patients need not be isolated; they can attend other patients and associate with non-immunes with perfect safety to the garrison. Nurses and attendants taking care of yellow fever patients shall remain isolated, so as to avoid any possible danger of their conveying mosquitoes from patients to non-immunes.

4. The infection of mosquitoes is most likely to occur during the first two or three days of the disease. Ambulant cases, that is, patients not ill enough to take to their beds and remaining unsuspected and unprotected, are probably those most responsible for the spread of the disease. It is therefore essential that all fever cases should be at once isolated and so protected that no mosquitoes can possibly get access to them until the nature of the fever is positively determined.

Each post shall have a 'reception ward' for the admission of all fever cases and an 'isolation ward' for the treatment of cases which prove to be yellow fever. Each ward shall be made mosquito-proof by wire netting over doors and windows, a ceiling of wire netting at a height of seven feet above the floor, and mosquito bars over the beds. There should be no place in it where mosquitoes can seek refuge, not readily accessible to the nurse. Both wards can be in the same building, provided they are separated by a mosquito-tight partition.

5. All persons coming from an infected locality to a post shall be kept under careful observation until the completion of five days from the time of possible infection, either in a special detention camp or in their own quarters; in either case, their temperature should be taken twice a day during this period of observation so that those who develop yellow-fever may be placed under treatment at the very inception of the disease.

6. Malarial fever, like yellow fever, is communicated by mosquito bites and therefore is just as much of an infectious disease and requires the same measures of protection against mosquitoes. On the assumption that mosquitoes remain in the vicinity of their breeding places, or never travel far, the prevalence of malarial fever at a post would indicate want of proper care and diligence on the part of the Surgeon and Commanding Officer in complying with General Orders No. 6, Department of Cuba, 1900.

7. Surgeons are again reminded of the absolute necessity, in all fever cases, to keep, from the very beginning, a complete chart of pulse and temperature, since such a chart is their best guide to a correct diagnosis and the proper treatment.

By Command of Major General Wood:

H. L. SCOTT,
Adjutant General.

CLIMATE AND CARBONIC ACID.*

By BAILEY WILLIS,
U. S. GEOLOGICAL SURVEY.

THE fact that a very extensive and massive ice sheet covered countries of the northern hemisphere which now enjoy a mild climate is generally known and accepted, although it is little more than fifty years since Agassiz (1840-47) made the then novel suggestion to explain the occurrence of glacial deposits where no glaciers remain. It is not so generally known that the great ice age was characterized by the development of several ice sheets in succession, each of them separated from its forerunner by an interval of mild climate during which the ice retreated far toward its source, and but few realize that these intervals of mildness were longer than the time which has elapsed since the latest glaciers withdrew from New England and the northern Central States.

Since the fact of a glacial period was established, several hypotheses have been framed to account for the phenomena of climatic change. As the sun warms the earth, variations in its condition and distance were postulated. As the poles are now regions of glacial accumulation, it was thought that the earth's axis of rotation might have shifted in such a way as to bring the once glaciated regions into polar relations. Or as heights of land are often mantled in snow and ice under latitudes where lowlands are free, glaciation was connected theoretically with a general elevation of continents and mountains. There are facts to sustain most of the speculations thus suggested. Each contains a possible cause. But no one is free from serious question of its sufficiency, while there is little evidence to show that any was definitely related in time to a glacial epoch, except that one which is based on a general elevation of the land.

Professor Chamberlin, of the University of Chicago, long ago advocated a method of investigation known as the method of multiple hypotheses. It calls upon the student to lay aside a natural preference for the theory which seems plausible and to consider as sincerely that which holds out small promise of development. As an earnest student of the causes of the glacial period, he has thus considered every suggestion that might solve that enigma. The astronomical causes, the shifting of the pole, the variations in altitude of the continent, have all been passed in review.

* A review of Chamberlin's 'Working Hypothesis of a Cause of Glacial Epochs.'

In considering the climatic conditions which gave to the coast of southern New England the aspect of Greenland at the present time, thought naturally turned to the antithetic phase when Greenland possessed the climate of Florida. And seemingly linked with these were other climatic variations, such as the great humidity of the Coal Measure period and the great aridity of epochs when salt and gypsum deposits accumulated; while the cause of that redness, which in several continents is characteristic of strata of certain geologic ages, might be traced to world-wide atmospheric conditions. The problem was thus greatly broadened in the scope of related phenomena, and the demands to be met by an adequate hypothesis became correspondingly complex.

The investigation upon which the hypothesis under review rests considers the physics and chemistry of the atmosphere in relation to temperature, the physics and chemistry of the ocean, the interaction of the ocean and the air, and those events of geologic history which as cause or effect may be related to the constitution of the atmosphere. It is not here proposed to review critically the several articles in which Professor Chamberlin and his associates have presented the results of profound researches. Suffice it to endeavor clearly to present an outline of their reasoning and conclusions.

The constitution of the atmosphere has long been known, and in a general way is stated for dry air as 21 parts of oxygen and 79 parts of nitrogen by volume. Argon, a newly discovered component, was formerly measured as nitrogen, and frequently there are impurities, though in small amount. There are 3 to 4 parts of carbonic acid in 10,000, and under natural conditions moisture is present in greater or less proportion. It is with these last, the carbonic acid and moisture, that the student of climatic changes has to deal chiefly.

The functions of carbonic acid and moisture in the atmosphere are threefold. They both absorb radiant heat in an unusual degree. By thus raising the temperature of the air, they both increase its capacity for moisture. And they both are chemically active.

Radiant light and heat penetrate the atmosphere to reach the solid earth, and are in part radiated back through the air into space. As the air is transparent toward light, so is it also toward heat, allowing both forms of energy to pass with moderate absorption. A photographer who compares the exposure of his plate at a considerable altitude with that near sea level roughly measures the relative strength of light at the two places and finds it less beneath the greater depth of atmosphere. The direct heat of the sun's rays is correspondingly less by the sea. The energy which the heated earth radiates back toward space is in part also absorbed by the air, which is thus warmed by the passage of rays to and from the earth.

In this absorption the mass of nitrogen and oxygen has but an

insignificant part. They are nearly perfectly transparent to heat. Carbonic acid and moisture are the effective constituents, which thicken, as it were, the atmospheric blanket, and being warmed in turn keep warm the earth. If they are decreased the blanket becomes thin and the surface grows cool.

Tyndall first suggested that a lessening of the proportion of carbonic acid might suffice to bring on the cold climate of a glacial epoch. He was followed by several investigators who determined more accurately the parts played by carbonic acid and by moisture, Austrian, German and American scientists competing in the study. In 1896, Dr. Arrhenius, a Swedish physicist, reached definite quantitative estimates of the effects. Employing values for the radiant heat of the full moon at different heights above the horizon, measured by Langley, he computed the heat absorbed by the atmosphere. By elaborate calculations he determined that a decrease of carbonic acid in the atmosphere to an amount ranging from 55 to 67 per cent. of the present content would reduce the average temperature 4 or 5 degrees C., which would bring on a glacial epoch, whereas an increase of carbonic acid to an amount two or three times the present content would elevate the average temperature 8 or 9 degrees C., and bring on a mild climate in high latitudes.

The effects of relatively absorbent or transparent atmospheres are not direct and uniform. They vary with the angle of incidence of the sun's rays and, therefore, with latitude, with seasons, and with day and night. They differ with altitude above the earth's surface; they are unlike on land and sea. But in general result the effect of greater absorptive power is to equalize all differences due to geographical and astronomical relations, whereas that of a relatively transparent condition is to accentuate them.

The physicist having thus indicated a possible solution, the further task of framing a working hypothesis was the geologist's. Chamberlin says: "There are hypotheses and working hypotheses. . . . General suggestions of a possible cause do not reach the dignity of working hypotheses until they are given concrete form, are fitted in detail to the specific phenomena and are made the agents of calling into play effective lines of research." In his attempt to frame a working hypothesis of the cause of glacial periods on an atmospheric basis, he has nobly met the requirements of his own definition under the difficult conditions imposed by the phenomena of glaciation. However the resulting working hypothesis may hereafter be modified by further research, its presentation must always stand as an example of the highest scientific effort.

Let us briefly review the requirements of the task. The fundamental postulate of the hypothesis is that variations of the atmospheric

content of carbonic acid have been the direct cause of variations of climate. It is necessary, therefore, to assign agencies adequate to bring about such alternations of poverty and wealth of carbonic acid. The agencies must operate to produce great cycles of climatic change which are recognized through study of the geologic record. Among others, these comprise an ancient event of extensive glaciation in India, Australia and South Africa, closely following the period of mild climate during which the Coal Measure flora flourished. The agencies must further promote subsidiary action by which minor oscillations of climate may be explained, since within the latest, the Pleistocene, period of glaciation, at least five, and probably more, advances of the ice occurred in alternation with intervals of comparative mildness, during which the ice retreated notably. Depletion and enrichment of the atmosphere must furthermore occur within reasonable limits of geologic periods. And cause must be shown why the atmospheric changes promoted glaciation about peculiarly local centers. In searching the sources of carbonic acid, Chamberlin has been led to reconsider the original constitution of the atmosphere, and thus also theories of the origin of the earth, including the nebular hypothesis. Thither this review may not follow him, but it will be of interest to advert to his views as to the conditions affecting biologic evolution, which are also causally related to variations of the carbonic acid contained in the air.

Carbonic acid, or as it is more accurately called, carbon dioxide, CO_2, occurs in many relations and plays many parts in the economy of the world. In some of these activities it enters into permanent combinations and is lost to the atmosphere. In others it passes through cycles of combination and release by which it is temporarily withdrawn from and subsequently returned to the air. If the atmosphere's resources in CO_2 be compared with a bank account, we may suppose that the balance follows one or the other of two familiar cases. In the one example there may have been originally a definite though possibly large deposit, which has not since been added to, but upon which many drafts have been and are being drawn. Under this assumption, however rich the atmosphere once was, it is now by comparison poverty stricken. On the other hand, there is reason to believe that the original capital in CO_2 was not materially greater than it is now, but that losses have been nearly balanced by gains. The first example represents a view held by geologists who believe that the atmosphere was exceedingly dense, moist and charged with carbon dioxide in early ages of the earth's history; the second illustrates the conceptions based on modern advances of biology and geology, and its acceptance is essential to the hypothesis of glaciation here discussed.

The sources from which fresh contributions may be made to the atmosphere are suggested by the occurrence of carbon dioxide among

the gases projected by the sun beyond its gravitative control, in interstellar spaces, in meteorites and in the terrestrial mass. The first three possible sources are too indefinite both in amount and in distribution through the ages to be of any present value to the hypothesis, but the last is important. Crystalline rocks of the superficial crust of the earth are shown by analysis to contain four and one-half times their own volume of gases, of which carbon dioxide, CO_2, and monoxide, CO, form a large percentage. During volcanic eruptions gases and vapors are ejected in indefinitely large volumes. What part of these was once of the atmosphere and is returned to it after an underground journey we do not know, but it is believed that a large part may come from the interior. The escape of these internal gases may also occur in some degree continuously by diffusion, and in influential amounts during episodes of mountain growth, when rock masses are strained and riven and upraised. We shall see presently that in the wasting of a mountain range there is serious consumption of carbon dioxide, which in greater or less degree temporarily affects the gain. The Pleistocene glaciation is attributed to a very notable offset of this character, but the exceptional nature of that event as compared with the relatively frequent episodes of mountain growth indicates that the gain of carbon dioxide has commonly equaled or exceeded the resulting temporary loss.

Among the cycles of combination and release, through which carbon dioxide runs, there are two which are both important, though not equally so. The first and less important is the cycle of organic change, involving plant and animal tissues. When grass grows, carbon dioxide is taken from the air. Grass becomes beef, and beef, through various changes, is resolved into new compounds, yielding back the carbon dioxide to the air. In its brief phases this cycle has no import for the hypothesis, but there are occasions where it is prolonged, as in the accumulations of vegetal substances fossilized as coal. The total amount of carbon dioxide thus abstracted, and now withheld, is very large, and is believed to have been an important factor in promoting at least one instance of glaciation, that which followed closely upon the Coal Measure period.

The more important cycle of combination and release involves the decomposition of rocks by weathering, the solution of certain products and their transportation to the sea, and the reactions through physical, chemical and organic agencies, by which carbon dioxide is either permanently locked up in limestones or is returned to the air.

For the purposes of this statement, the common minerals of rocks may be classified as silicates and carbonates, that is as compounds with silicic acid and compounds with carbonic acid. The former may be typified by the familiar minerals of granite, the latter by limestone.

Granite and all similar crystalline aggregates of silicates disintegrate, and the separate minerals are decomposed chemically by the action of carbon dioxide and moisture. Of the various compounds which result, those of carbon dioxide with lime and magnesia are of most direct interest in this connection, and those with lime may be discussed as representative.

The common combinations of lime and carbon dioxide are two: the carbonate of lime, more specifically called the normal or monocarbonate, and the bicarbonate of lime. The carbonate consists of one ion, or chemical unit, of lime, CaO, combined with one ion of carbon dioxide, CO_2. The bicarbonate consists of one ion of lime combined with two ions of carbon dioxide. The carbonate is but slightly soluble in water, the bicarbonate is easily dissolved. The carbonate is produced in the decomposition of silicates, and great amounts of it which have been derived from this source in past ages are now contained in limestones and other calcareous sedimentary rocks. Whether it exists for a brief time in the weathering of silicates or is, as limestone, exposed to atmospheric waters, the carbonate very readily combines with carbon dioxide, and the bicarbonate is formed in solution. All surface and underground waters contain bicarbonate of lime in greater or less quantity, and enormous volumes are annually conveyed to the sea. It is estimated roughly that the weight of the carbonate of lime thus dissolved and contributed to the sea annually is 2,700,000,000 tons. This is about one-half of the total saline matter dissolved in surface waters annually, and a portion of the remainder consists of carbonates of magnesia, potash and soda.

It has been computed by Professor Chamberlin and his associates that the present supply of carbon dioxide in the atmosphere would be exhausted by the decomposition of silicates in 5,000 to 18,000 years at the present rate of consumption if there were no source of replenishment. It is evident that the amount of carbon dioxide in the atmosphere at any time is the balance between supply and draft, and that it may be more or less as one or the other preponderates. The next step in forming the hypothesis, therefore, is to consider conditions which may produce fluctuations of consumption and contribution.

The consumption of carbon dioxide in weathering of rocks is an effect of erosion, the familiar process which tends to reduce heights of land to a low slope, declining to sea level. This tendency is opposed by those internal forces of the earth's mass which depress sea bottoms and relatively uplift continents and mountain ranges. The persistent attacks of the sun's energy are directed against the earthworks raised by terrestrial forces. It is the fabled fight of the powers of light and air against the powers of the dark underworld; and the former never pause, whereas the latter sleep for ages, and, awaking, exert themselves

mightily for a brief time only. While they rest, mountains waste to hills and hills to plains, and the sea spreads over the margins of sinking continents. When they put forth their strength, mountains grow and continents rise from the waters. Their intermittent activity exerts a potent influence on the constitution of the atmosphere, and is so important to the hypothesis of glaciation that a more definite account of the evidence of its periodic nature is necessary.

Sediments laid beneath the sea are waste of continents. By their characters and volume they may indicate their derivation from far-stretching lands or from near mountains. They often occur spread across the bases of ranges which have been planed away by air and waves. By evidences such as these the physical history of any province may be made out; by comparison of provinces the major events in the history of a continent are ascertained; and by comparison of continental histories the sequence of world stages is studied. For any province the limits of knowledge depend only on the completeness of the record in the local rock series; for each continent the inferences are qualified by difficulties of correlating successive steps from province to province; and world-wide conclusions must necessarily be restricted to the broadest effects.

In the present condition of geologic investigation we know but incompletely the rhythm of continental and marine oscillation, but certain marked epochs are recognized. Seas were extensive, while lands were low and restricted, during epochs known to geologists as the middle Ordovician, the middle Silurian, the early Carboniferous, the late Jurassic and the upper Cretaceous. At these times the consumption of carbon dioxide by rock weathering was comparatively slight, according to hypothesis. On the other hand, lands were wide and seas confined to their basins during the close of the Silurian and beginning of Devonian time, during the Permian and early Triassic periods, and during the Pliocene and Pleistocene. These were epochs of unusual consumption of carbon dioxide.

The climatic effect of depletion of carbon dioxide depends upon the rate at which it is taken from the atmosphere. If it were abstracted slowly a large loss might be compensated by moderate supply, whereas if it were rapidly removed the effect on the atmospheric content might be decided. In this relation the growth of mountains has an important accelerating influence. Although the rate of weathering is conditioned by many factors, elevation is so important that Chamberlin's estimate is probably near the truth. It is that for continental areas the rate of carbonation varies probably more nearly as the square than as the simple ratio of altitudes.

Modern studies of mountain growth have materially changed the views held within a decade by geologists as to the ages of ranges.

Among the rocks of any range there are youngest strata that mark a date earlier than the most remote at which the uplift may have occurred. Impressed with the magnitude and grandeur of mountains, geologists assigned them an antiquity limited only by the age of their component strata, but through the interpretation of landscape forms evidence is now accumulating to show that existing ranges are, as a rule, comparatively young. One interesting conclusion is that we live at a time near the culmination of an epoch of mountain growth, that mountains are now widely distributed and high as compared with those of many preceding periods, and the earth's activity as thus manifested is not materially less now than formerly within known geologic history. The crustal adjustment which produced existing mountain ranges and expanded continents appears to have culminated just before or very early in the Glacial epoch, and the recognition of this fact was the principal basis for the hypothesis that glaciation was related directly to elevation of the land areas. Chamberlin interprets the relation through the influence of rock-weathering upon the carbon dioxide of the air, and attributes the cold period to the resulting thermal transparency of the atmosphere.

The carbon dioxide abstracted from the air by weathering passes into the aqueous circulation of the globe, one-half of it in combination as monocarbonates, the other half superadded to form bicarbonates. A further step in framing the hypothesis is to follow this second part until it shall be returned to the air, which shall thus be reenriched and may promote a period of mild climate.

The ocean is a great reservoir holding carbon dioxide in combination with various bases as bicarbonate. It contains also many other salts. Assuming that all the solids dissolved in sea-water have been derived from the land at a rate of solution equal to that now determined by analyses of river waters, it is possible to make a curious calculation, which shows that the carbonate of lime now in the sea would have accumulated in 60,000 years, whereas the common salt, chloride of sodium, would have required 166,000,000 years. The common salt is not removed from solution, nor is there reason to suppose that there is any special source from which it is concentrated, but which does not supply lime; it may, therefore, be taken as a standard of comparison, which shows that there is much less lime in the sea than we should expect. The deficit is accounted for by the great beds of limestone deposited from the sea at various periods from the long past to the present.

In the ocean, bicarbonate of lime is dissolved in a proportion less than that which the waters can hold in solution, and, according to the principles of the older chemistry, it is under these conditions a fixed combination, which remains dissolved. Should, however, the mono-

carbonate of lime separate from the water as a solid precipitate, the second part of the carbon dioxide would be free to pass from the water into the atmosphere. We are thus led to consider the agencies which have caused the deposition of lime from dilute solution.

Modern views of chemical combinations regard compounds in solution as going through a constant interchange of reactions, by which ions pass continuously from one association to another, as in the grand chain the dancers weave in and out with touch of hands. The dance of the ions, more technically called dissociation, is most active in dilute solutions, and is promoted by higher, retarded by lower temperatures. It has been shown by experiment that bicarbonate of lime may be dissociated by agitating the solution, and there are occurrences of calcareous formations which indicate that the monocarbonate is deposited as a result of such action. Thus it is probable that, through this process, warm seas surrender to the air a notable amount of carbon dioxide, but that the contribution becomes insignificant or ceases when the waters are chilled.

Under favorable conditions the ocean abounds in organisms which secrete normal carbonate of lime as parts of their structures. They swarm in the warm waters of tropical oceanic currents, they exist in multitudes on the warm shallows where the sea spreads over the margins of continental masses with a depth not exceeding 100 fathoms, but they are rare or are replaced by species without hard parts in cold waters. The physiological reactions by which these organisms obtain the normal carbonate from the water are not definitely known. They may take it from bicarbonate in solution, or by reaction on sulphate of lime setting free sulphuric acid, which attacks the bicarbonate. In any case, the effect is to fix one ion of carbon dioxide in the solid normal carbonate, and to free the second ion, which may pass into the air. The enormous volume of organic calcareous deposits now forming, and the massive limestone strata of past ages, largely or wholly of organic origin, attest the importance of the process. Life may be considered the most important of those agencies which restore carbon dioxide to the atmosphere, but it is narrowly conditioned by limitations of habitat and warmth.

Carbon dioxide absorbed in sea-water is yielded to the atmosphere and returned by it under varying conditions of tension of the gas, of barometric pressure, and of temperature. At moderate temperature the sea gives up the gas freely, and would supply a deficiency gradually brought about in the atmosphere. But colder waters hold it faster, and may even take carbon dioxide from an already depleted atmosphere.

Thus the processes of dissociation by chemical and organic agencies and of absorption depend upon temperature, and through this dependence promote the prevailing tendency of climatic changes. If the

change be from warm to cold, cooling waters tighten their grasp on the precious gas that might offset the atmospheric depletion. If they be warmed beneath an air growing rich in carbon dioxide, they become generous of their hoard. The processes are, therefore, auxiliary and intensifying, not initiative.

For the initiative process, which may start the train of effects leading to atmospheric enrichment and a warm epoch, we must refer again to the periodic rest and unrest of the earth's forces.

When, through adjustment of the relations between continental masses and masses beneath the oceans, the internal stresses of the globe have been balanced, the average elevation of lands above sea level is a maximum. The highest rate of consumption of carbon dioxide by weathering may be assumed to follow after a brief but appreciable interval, and from that time forth to diminish. As heights waste and slopes sink low, they become mantled with the residual product of weathering, soil, and efficient contact of carbon dioxide with unaltered rock is limited. When the average height of land is become that of a low plain, carbonation is reduced to a very small part of its maximum activity, and the rate of consumption is slow. At some stage of this change the diminishing rate of depletion may equal and thereafter sink below the rate of supply. Thus the initial condition of a return of milder climate inheres in the transient nature of the cause of a cold epoch. The result might, however, be long delayed, but for the accelerating influence of auxiliary processes, of which, as already stated, life is believed to be the most potent.

It is a recorded fact of geologic history that periods of minimum continental elevation have been periods of extensive marine expansion. These conditions have been associated with remarkable development of marine faunas and with general mildness. Chamberlin was the first to point out a causal relation between these conditions and effects. He entertains the idea that at the climax of an epoch of crustal adjustment, the elevation of continents may be somewhat greater than that required by radial equilibrium. If so, they should in time exhibit a tendency to settle back. In so far as this period of readjustment of balance might suffice for deep general denudation, the subsiding lands would present low plain surfaces to the sea. These conditions would be most favorable for wide migration of the shore from the continental margins far inland, and would result in extensive areas of relatively shallow water. A fauna, which had existed on the narrow slope between the original position of the shore and an oceanic basin, would find its habitat immensely enlarged and favorably conditioned. Responding, it would develop varieties, species and genera until, as sometimes was the case, exuberance ran into unfitness and decadence.

The lowland aspect of continents would be favorable to other

ameliorating influences, as well as to life. Before it could have attained its later phases, the acute thermal transparency of the atmosphere must have given place to moderate absorption, and temperate conditions must have succeeded cold. From waters warmed on widening shallows, carbon dioxide would pass into the air by simple diffusion and by chemical dissociation. But the principal contribution, upon which generally prevailing mildness would attend, would be associated with the active development of lime-secreting life, and this relation is firmly established by observation.

Grand seasons of the eras are thus interpreted by Chamberlin as effects of periodic adjustments of the earth's superficial form to stresses developed within its mass. The causes of these stresses are sought by physicists and geologists in the most profound researches, and for the present, at least, they elude discovery, because the physical and chemical conditions of matter within the earth transcend conditions of observation. But geologic investigation is competent to trace their influence upon aspects of the earth, and not the least valuable result of Chamberlin's thought is the impulse it imparts to studies into the geography and life of the past.

The general hypothesis being thus promisingly developed, some would have been satisfied there to rest the suggestion, and the general reader may be content with the splendidly panoramic view of effects and causes which it embraces. But its author pursues its analysis and application with rigorous questioning, limited only by the bounds of existing knowledge, and where knowledge fails he points out the need of research. We shall touch only upon the principal points of his thorough discussion, the competency of the causes, the oscillations of glaciation, the time limits set by the probable duration of glacial and interglacial epochs and the localization of glaciation in Pleistocene and in Carboniferous times.

As already stated, the Pleistocene glaciation is attributed to depletion of the atmospheric carbon dioxide occasioned by the notable expansion and elevation of lands late in the Pliocene period. It is estimated that in the preceding warm age the land area was 44,000,000 square miles. That of the succeeding expansion at its maximum is computed at 65,000,000 square miles, and the present extent is taken at 54,000,000 square miles. That is to say, the areas are related nearly as $1:1\frac{1}{2}:1\frac{1}{4}$. Elevation, which is more important than extent, was at the time of greatest expansion at least two or three times what it shortly before had been when continents were smaller. In the earlier time of mildness the margins of continents were generally submerged, as the eastern portion of North America now is, affording a roomy habitat for lime-secreting marine life. But with the uplift of continents these sea-shelves were reduced to narrow zones along the steeps which descend

into oceanic depths. Low, limited lands and wide, warm seas had promoted the flow of carbon dioxide from the waters to the air. Lands elevated and expanded and seas shrunk within their basins reversed the course, and the earth took from the air to give to the waters.

The rate of depletion is capable of reasonable calculation. If the amount of carbon dioxide taken from the atmosphere exceeded by 10 per cent. that supplied to it from all possible sources, 50,000 years would suffice to reduce the content from .18 per cent. to .03 per cent. by weight. And this change would bring on glaciation. There are few students of the earth's history who would be willing to admit that the associated effects of topographic development could have occurred in less, if, indeed, in so short a time, and the causes assigned are thus seen to be fully equal to the task imposed.

The climates of the Glacial period were marked by rhythm recorded in advance and retreat, and re-advance and withdrawal, of the ice front several times repeated. The major changes were as great as that which has intervened between the severest glaciation and the present, and occurred early in the series. The later oscillations declined in both Europe and North America. Such rhythmic rebound from one phase to another and back again is characteristic of phenomena which, though they swing to extremes, themselves set up the action that reverses the movement. The ice sheet itself set the bounds of its possible spread.

Assuming glaciation to be inaugurated and the cold to be intensified by consequent accelerating influences, which need not be detailed here, the depleting process of weathering must be checked by the mantling ice and refrigeration. It is estimated that frost and ice at their maximum effect protected 20 per cent. of the Pleistocene land area. Continued depletion depended on the balance between contribution and abstraction, and it is suggested that 20 per cent. (or whatever may have been the proportion of land area sealed against carbonation) represented the initial preponderance of draft over supply. Whenever the effects of glaciation reduced the consumption of carbon dioxide below the inflow from all sources, the glacial epoch would end and the reaction would begin. Once initiated, it would be accelerated by diffusion and dissociation in the richly stored seas, and by renewed development of life in the warmer waters. The mildness might increase till the great glaciers had vanished, but it could have come to stay only in case the height and area of land had adequately diminished.

Lands remained extensive and elevations great during the Pleistocene period. They were even wider and higher than they are now. As an early ice mantle shrunk it bared rock masses and glacial deposits, which were to a great extent favorably conditioned for chemical attack. The renewed consumption of carbon dioxide in time overbalanced the supply, and glaciation went on again.

What was the period of this climatic pendulum? The answer comes to us vaguely in echoes of Niagara's voices. The cataract began its existence at Lewiston during the retreat of the latest ice sheet. Since that time the gorge has been cut back from Lewiston to the present site of the falls, and it is possible to estimate roughly what time the task has consumed. This episode is one-half or less than one-half of the time elapsed since the beginning of the retreat of the ice from its most advanced position. Thus indefinitely, we may count that something like 40,000 years sped while the climate changed from those Greenland conditions to these which we now enjoy. By similar conservative studies of the effects of deposition and erosion accomplished before the latest glaciation, the duration of the interglacial epoch is found to be several times that of the post-glacial interval; that is, in numbers, 80,000 or 120,000 years or more.

The significance of these figures does not depend upon their precision. They confessedly do but indicate the general magnitude of the times. But they serve to show that those times were more than sufficient for the operation of the causes assigned to produce the observed effects, and thus they sustain the hypothesis. Furthermore, they serve to bring glaciation near to us. In earth history, whose eras are measured by millions of years, events which occurred a hundred thousand years ago are of recent date. We live within the operation of the causes which may hinder or promote glaciation, and, though the present is an age of comparative mildness, we cannot be sure whether this be the spring of a great era or midsummer of an epoch. Are the gnomes of the under-world wearied of mountain building, and have they sunk to rest? Are the shafts of the sun's heat as they traverse the air effectively caught and stored? Does man, consuming fossil carbon in his manifold activities, unconsciously postpone the return of winter?

The cause of a glacial epoch may be found when an adequate cause of cold is linked with the occurrence of glaciation, but the spread of an ice mantle is dependent on snowfall as well as on temperature, and it is through this relation that the peculiar distribution of Pleistocene glaciers may be explained. It will suffice here to state the meteorological conditions which, according to Chamberlin, determined the most striking centers of accumulation, those which were situated in the plains of north-northeastern America.

Studies of polar currents, which free the northern coast of Siberia of ice and crowd it upon the American Arctic Archipelago, combined with the partial data available as to the barometric conditions of the Arctic zone, lead him to the conclusion that in the northern hemisphere the grand movement of the atmosphere from west to east about the globe is oblique to parallels of latitude, and is upon an axis which has

its pole at a point located somewhere north of Hudson Bay. One effect of this oblique rotation is to establish in northern latitudes between 50 and 60 degrees two areas of low barometer and one of high barometer, a disposition which is in strong contrast to the condition in the southern hemisphere, there being a zone of high pressure along the parallel of 35 degrees, south latitude, with decreasing pressure thence toward the Antarctic. These lows and highs differ from those which are familiar as features of daily weather maps, in that they are nearly stationary. The well-known migrant centers converge toward and run into the great fixed centers. The two permanent lows are situated one across the North Atlantic, from Hudson Bay to Scandinavia, the other in the North Pacific, from Japan to southern Alaska. They are centers of inflowing ascending air currents, and are, therefore, characterized by great precipitation. The region of maximum glaciation at the present time lies between them; one conspicuous development occurring in Greenland in the northwest quarter of the Atlantic low, another lying in Alaska in the northeast quarter of the Pacific low. By an analysis of the winds, it is shown that both Greenland and Alaska lie to leeward of the prevailing currents where they pass ashore. They are not necessarily the provinces of maximum precipitation, rain and snow both considered, but they are areas of copious snowfall, with low annual mean temperatures.

The northern high lies nearly midway between the two lows over Siberia. In contrast to them, it is a center of descending outward-flowing currents, marked by slight precipitation, and it is not now, nor was it in Pleistocene time, a scene of glacial development.

The centers of Pleistocene glaciation were so arranged with reference to the glacial regions of to-day that they would be determined by the oblique circulation and distribution of areas of low pressure, if existing conditions were intensified. An adequate occasion of intensification is found in the thermal transparency of the atmosphere, resulting from depletion of carbon dioxide, and thus the localization of Pleistocene ice sheets is explained in a manner consistent with the major hypothesis of the cause of glaciation.

Chamberlin's hypothesis is framed on an atmospheric basis, but the efficiency of the agencies which it postulates depends upon geographic conditions, upon distribution of land and sea and average heights of continents. The geography of the earth in the closing epochs of the Paleozoic era is known only in its broadest outlines, and they are but vaguely traced. With such imperfect data it is impracticable to explain satisfactorily the extraordinary phenomena of glaciation at that date in intimate association with the development of coal beds and extending within the tropics. Nevertheless, to carry out his purpose of developing a working hypothesis, the author feels obliged to arrange

the known facts and to discuss them along the lines so successfully followed in reference to Pleistocene glaciation. An important difference in the argument as applied to the two cases lies in the cause assigned for depletion of the atmospheric carbon dioxide. The postulate of an appropriate time relation between Paleozoic land movements and the epoch of glaciation being conservatively assigned a minor position, the storage of carbon as coal is given major rank in accordance with known relations. This process is not attended by the accelerating and reacting influences, which are due to the equivalent of carbon dioxide contained in bicarbonates, and glaciation would, therefore, result only after depletion had continued longer. In this suggestion is found a possible reason for the wide extension of cool climate and the occurrence of glaciation in remarkably low latitudes.

The broad scope of philosophic thought upon which this working hypothesis rests is indicated by the titles of articles which have flowed from Chamberlin's pen in the last four years.* Fortifying his own general researches where needed by those of specialists, he, with reason, challenges fundamental and generally accepted views. He gives the geologist and biologist new clues with which to thread the labyrinth of knowledge, and develops important relations between dynamical geology, stratigraphy, climate and evolution.

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* A Group of Hypotheses bearing on Climatic Changes, Jour. Geol., Vol. V., No. 7, 1897. The Ulterior Basis of Time Divisions and the Classification of Geologic History, ibid., Vol. VI., No. 5, 1898. A Systematic Source of Evolution of Provincial Faunas, ibid., No. 6, 1898. The Influence of Great Epochs of Limestone Formation upon the Constitution of the Atmosphere, ibid. Lord Kelvin's Address on the Age of the Earth as an Abode Fitted for Life, Science, N. S., Vol. IX., No. 235, pp. 889-901, June 30, 1899, and Vol. X., No. 236, pp. 11-18, July 7, 1899.

THE PEOPLING OF THE PHILIPPINES.*

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SINCE the days when the first European navigators entered the South Sea, the dispute over the source and ethnic affiliations of the inhabitants of that extended and scattered island world has been unsettled. The most superficial glance points out a contrariety in external appearances, which leaves little doubt that here peoples of entirely different blood live near and among one another. And this is so apparent that the pathfinder in this region, Magellan, gave expression to the contrariety in his names for tribes and islands. Since dark complexion was observed on individuals in certain tribes and in defined areas, and light complexion on others, here abundantly, there quite exceptional, writers applied Old World names to the new phenomena without further thought. The Philippines set the decisive example in this. Fernando Magellan first discovered the islands of this great archipelago in 1521, March 16. After his death the Spaniards completed the circle of his discoveries. At this time the name of Negros was fixed,† which even now is called Islad de los Pintados. For years the Spaniards called the entire archipelago Islas de Poniente; gradually, after the expedition of Don Fray Garcia Jofre de Loaisa (1526), the new title of the Philippines prevailed, through Salazar. The people were divided into two groups, the Little Negroes or Negritos and the Indios. It is quite conceivable that involuntarily the opinion prevailed that the Negritos had close relationship with the African blacks, and the Indios‡ with the lighter-complexioned inhabitants of India, or at least of Indonesia.

However, it must be said here that the theory of a truly African origin of the Negritos has been advanced but seldom, and then in a very hesitating manner. The idea that with the present configuration

* Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin. Berlin, 1897, January-June, 279-289. Translated with notes by Professor O. T. Mason for the annual report of the Smithsonian Institution, and printed from an advance copy supplied by Professor Samuel P. Langley, secretary of the Institution.

† NOTE.—The island of Negros received its name because it was peopled chiefly by a dark, woolly-haired race, while in other islands these were confined to the interior. Cf. A. B. Meyer, Negritos, 1899, p. 16.—TRANSLATOR.

‡ This word, except in an historical sense, should never be used for non-Negrito Filipinos.

of the eastern island world, especially with their great distances apart, a variety of mankind that had never manifested any aptitude for maritime enterprises should have spread themselves over this vast ocean area, in order to settle down on this island and on that, is so unreasonable that it has found scarcely a defender worth naming. More and more the blacks are coming to be considered the original peoples, the 'Indios' to be the intruders. For this there is a quite reasonable ground, in that on many islands the blacks dwell in the interior, difficult of access, especially in the dense and unwholesome mountain forests, while the lighter complexioned tribes have settled the coasts. To this are added linguistic proofs, which place the lighter races, of homogeneous speech, in linguistic relations with the higher races, especially the Malays. Dogmatically it has been said that originally these islands had been occupied entirely by the primitive black population, but afterwards, through intrusions from the sea, these blacks were gradually pressed away from the coast and shoved back into the interior.

The problem, though it appears simple enough, has become complicated more and more through the progress of discovery, especially since Cook enlarged our knowledge of the oriental island world. A new and still more pregnant contrast then thrust itself to the front in the fact that the blacks and the lighter-colored peoples are each separated into widely differing groups. While the former hold especially the immense, almost continental, regions of Australia (New Holland) and New Guinea, and also the larger archipelagos, such as New Hebrides, Solomon Islands, Fiji (Viti) Archipelago—that is, the western areas—the north and east, Micronesia and Polynesia, were occupied by lighter-colored peoples. So the first divison into Melanesia and Polynesia has in latest times come to be of value and the dogma once fixed has remained. For the Polynesians are by many allied to the Malays, while the blacks are put together as a special ethnological race.

For practical ethnology this division may suffice. But the scientific man will seek also for the blacks a genetic explanation. The answer has been furnished by one of the greatest ethnologists, Theodor Waitz,* who, after he had exposed the insufficiency of the accepted formulas, came to the conclusion that the differentiation of the blacks from the lighter peoples might be an error. He denied that there had been a primitive black race in Micronesia and Polynesia; in his opinion we have here to do with a single race. The color of the Polynesians may be out and out from natural causes different; indeed, 'their entire physical appearance indicates the greatest variability.' Herein the

* Anthropologie der Naturvölker, Vol. V; The South Sea Islanders, Part II; The Micronesians and Northwestern Polynesians. Leipzig, 1870, pp. 33-36. Finsch, Verh. d. Berliner Anthrop. Ges., 1882, p. 164.

whole question of the domain of variation is sprung with imperfect satisfaction on the part of those travelers who give their attention more to transitions than to types. Among these are not a few who have returned from the South Sea with the conviction that all criteria for the diagnosis of men and of races are valueless.

Analytical anthropology has led to other and often unexpected results. It has proved that just that portion of South Sea population which can apparently lay the strongest claim to be considered a homogeneous race must be separated into a collection of subvarieties. Nothing appears more likely than that the Negritos of the Philippines are the nearest relatives to the Melanesians, the Australians, the Papuans; and yet it has been proved that all these are separated one from another by well-marked characters. Whether these characters place the peoples under the head of varieties, or whether, indeed, the black tribes of the South Sea, spite of all differences, are to be traced back to one single primitive stock, that is a question of prehistory for whose answer the material is lacking.* Were it possible to furnish the proof that the black populations of the South Sea were already settled in their present homes when land bridges existed between their territory and Africa, or when the much-sought Lemuria still existed, it would not be worth the trouble to hunt for the missing material. In our present knowledge we can not fill the gaps, so we must yet hold the blacks of the Orient to be separate races.†

The hair furnished the strongest character for diagnosis, in which, not alone that of the head is under consideration; the hair, therefore, occupies the foreground of interest. Its color is of the least importance, since all peoples of the South Sea have black hair. It is more the structure and appearance which furnish the observer convenient starting points for the primary classification. Generally a twofold division satisfies. The blacks, it is said, have crisped hair, the Polynesians and light-colored peoples have smooth hair. But this declaration is erroneous in its generality. It is in no way easy to declare absolutely what hair is to be called crisp, and it is still more difficult to define in what respects the so-called crisp varieties differ one from another. For a long time the Australian hair was denominated crisp, until it was evident that it could be classed neither with that of the Africans nor with that of the Philippine blacks. Semper, one of the first travelers to furnish a somewhat complete description of the physical characters of the Negritos, describes it as an "extremely thick, brown-black,

* Note.—The reader must consult, on the identity of Negritos with Papuans, A. B. Meyer in Zeitschrift für Ethnologie, Verhandl., Berlin, 1875, p. 47, and the Distribution of the Negritos, Dresden, 1899, pp. 76-87.—Tr.

† On Lemuria cf. A. R. Wallace, Geog. Distrib. of Animals, 1876, I, p. 272, and Island Life, 1880, p. 394.—Tr.

lack-luster, and crisp-woolly crown of hair."* Among these peculiarities the lack-luster is unimportant, since it is due to want of care and uncleanliness. On the contrary, the other data furnish true characters of the hair, and among them the crisp-woolly peculiarity is most valuable.

On the terms 'wool' and 'woolly' severe controversies, which have not yet closed, have taken place among ethnologists during the last ten years. Also the lack of care, especially the absence of the comb, has here acted as a disturbing cause in the decision. But there is yet a set of peoples, which were formerly included, that are now being gradually disassociated, especially the Australians and the Veddahs, whose hair, by means of special care, appears quite wavy if not entirely sleek and smooth. Generally it is frowzy and matted, so that its natural form is difficult to recognize. To it is wanting the chief peculiarity, which obtrudes itself in the African blacks so characteristically that the compact spiral form which it assumes from its root, the so-called 'pepper-corn,' is selected as the preferable mark of the race. The peculiar nappy head has its origin in the spiral '*rollchen*.' As to the Asiatic blacks this has been for a long time known among the Andamanese; it has lately been noticed upon the Sakai of Malacca, and it is to be found also among the Negritos of the Philippines, as can be shown by specimens. Therefore, if we seek ethnic relationships for the Negritos of the Philippines, or as they are named, the Aetas (Etas, Itas), such connections obtrude themselves with the stocks named, and the more strongly since they all have brachycephalic, relatively small (nannocephalic) heads and through their small size attach themselves to the peculiar dwarf tribes.

I might here comment on the singular facts that the Andaman Islands are situated near the Nicobars in the Indian Ocean, but that the populations on both sides of them are entirely different. In my own detailed descriptions which treat of the skulls and the hair specially,† it is affirmed that the typical skull shape of the Nicobarese is dolichocephalic and that "their hair stands between the straight hair of the Mongoloid and the sleek, though slightly curved or wavy, hair of the Malayan and Indian peoples"; their skin color is relatively dark, but only so much so as is peculiar to the tribes of India. With the little blacks of the Andamans there is not the slightest agreement. In this we have one of the best evidences against the theory of Waitz-Gerland that the differences in physical appearance are to be attributed to variation merely. I will, however, so as not to be misunderstood, expressly emphasize that I am not willing to declare

* Die Philippinen und ihre Bewohner, Würzburg, 1869, p. 49.

† Verhandl. der Berliner Anthrop. Gesellschaft, 1885, pp. 104, 109.

that the two peoples have been at all times so constituted; I am now speaking of actual conditions.

In the same sense I wish also my remarks concerning the Negritos to be taken. Not one fact is in evidence from which we may conclude that a single neighboring people known to us has been Negritized. We are therefore justified when we see in the Negritos a truly primitive people. As they are now, they were more than three hundred and fifty years ago when the first European navigators visited these islands. About older relationships nothing is known. All the graves from which the bones of Negritos now in possession were taken belong to recent times, and also the oldest descriptions which have been received, so far as phylogeny is concerned, must be characterized as modern. * * *

Whoever would picture the present ethnic affiliations of the light-colored peoples of the Philippines will soon land in confusion on account of the great number of tribes. One of the ablest observers, Ferd. Blumentritt,* mentions, besides the Negritos, the Chinese and the whites, not less than 51 such tribes. He classifies them in one group as Malays, according to the plan now customary. This division rests primarily on a linguistic foundation. But when it is noted that the identity of language among all the tribes is not established and among many not at all proved, it is sufficiently shown that speech is a character of little constancy, and that a language may be imposed upon a people to the annihilation of their own by those who belong to a different linguistic stock. The Malay Sea is filled with islands on which tarry the remnants of peoples not Malay.

For a long time, especially since the Dutch occupation, these old populations have received the special name of Alfuros.† But this ambiguous term has been used in such an arbitrary and promiscuous fashion that latterly it has been well-nigh banished from ethnological literature. It is not long ago that the Negritos were so called. But if the black peoples are eliminated, there remains on many islands at least an element to be differentiated from the Malay, chiefly through the darker skin color, greater orthocephaly, and more wavy, quite crimped hair. I have, for the different islands, furnished proof, and will here only refer to the assertion that "a broad belt of wavy and curly hair has pressed itself in between the Papuan and the Malay, a belt which in the north seems to terminate with the Veddah, in the south with the Australian." One can not read the accounts of travelers without the increasing conviction of the existence of several different,

* Versuch einer Ethnographie der Philippinen, Petermann's Mittheilungen, Gotha, 1882, No. 67.

† A. Lesson. Les Polynesians, Paris, 1880, Vol. I, pp. 267, 283. [On this objectionable word see A. B. Meyer, The Distribution of the Negritos, Dresden, 1899, Stengel, p. 7.—Tr.]

if not perhaps related, varieties of peoples thrust on the same island.

From this results the natural and entirely unprejudiced conclusion, which has repeatedly been stated, that either a primitive people by later intrusions has been pressed back into the interior or that in course of time several immigrations have followed one another. At the same time it is not unreasonable to think that both processes went on at the same time, and indeed this conception is strongly brought forward.* So Blumentritt assumes that there is there a primitive black people and that three separate Malay invasions have taken place. The oldest, whose branches have many traits in accord with the Dayaks of Borneo, especially the practice of head-hunting; a second, which also took place before the arrival of the Spaniards, to which the Tagals, Visayas, Vicols, Ilocanes, and other tribes belong; the third, Islamitic, which emigrated from Borneo and might have been interrupted by the arrival of the Spaniards, and with which a contemporaneous immigration from the Moluccas went on. It must be said, however, that Blumentritt admits two periods for the first invasion. In the earliest he places the immigration of the Igorrotes, Apayos, Zambales—in short, all the tribes that dwelt in the interior of the country later and were pressed away from the coast, therefore, actually, the mountain tribes. To the second half he assigns the Tinguianes, Catalanganes, and Irayas, who are not head-hunters, but Semper says they appear to have a mixture of Chinese and Japanese blood.†

Against this scheme many things may be said in detail, especially that, according to the apparently well-grounded assertions of Muller-Beeck, the going of the Chinese to the Philippines was developed about the end of the fourteenth century, and chiefly after the Spaniards had gotten a foothold and were using the Mexican silver in trade. At any rate, the apprehension of Semper, which rests on somewhat superficial physiognomic ground, is not confirmed by searching investigations. So the head-hunting of the mountain tribes, so far as it hints at relations with Borneo, gives no sure chronological result, since it might have been contemporaneous in them and could have come here through invasion from other islands.

The chief inquiry is this: Whether there took place other and older invasions. For this we are not only to draw upon the present tribes,

* R. Virchow, Alfuren-Schädel von Ceram und von den Molucken. Verhandl. Berl. Anthrop. Gesellschaft, 1882, p. 78; 1889, pp. 159, 170. [Whether this be a new type or mixture cf. J. G. F. Riedel, Kroesharige Rassen tuesschen Selebes en Papua, 1886.—Translator.]

† Note.—The dates for these several migrations are given as follows: First migration, 200 B. C.; second migration 100-500, A. D., bringing the alphabet; third migration, fourteenth and fifteenth centuries, Islamitic. But these dates represent only opinions up to date, from which more thorough inquiry must set out.—Translator.

but if possible upon the remains of earlier and perhaps now extinct tribes. This possibility has been brought nearer for the Philippines through certain cave deposits. We have to thank, for the first information, the traveler Jagor, whose exceptional talent as collector has placed us in the possession of rich material, especially crania. To his excellent report of his journey I have already dedicated a special chapter, in which I have presented and partially illustrated not only the cave crania, but also a series of other skulls. An extended conference upon them has been held in the Anthropological Society.*

The old Spanish chroniclers describe accurately the mortuary customs which were in vogue in their time. The dead were laid in coffins made from excavated tree trunks and covered with a well-fitting lid. They were then deposited on some elevated place, or mountain, or river bank, or seashore. Caves in the mountains were also utilized for this purpose. Jagor describes such caves on the island of Samar, west of Luzon, whose contents have recently been annihilated.†

The few crania from there which have been intrusted to me bear the marks of recent pedigree, as also do the additional objects. Unfortunately, Dr. Jagor did not himself visit these interesting caves, but he has brought crania thence which are of the highest interest, and which I must now mention.

The cave in question lies near Lanang,‡ on the east coast of Samar, on the bank of a river, it is said. It is, as the traveler reports, celebrated in the locality "on account of its depressed gigantic crania, without sutures." The singular statement is made clear by means of a well-preserved example, which I lay before you. The entire cranium, including the face, is covered with a thick layer of sinter, which gives it the appearance of belonging to the class of skulls with Leontiasis ossea. It is, in fact, of good size, but through the incrustation it is increased to gigantic proportions. It is true, likewise, that it has a much flattened, broad and compressed form. The cleaning of another skull has shown that artificial deformation has taken place, which obviously was completed before the incrustation was laid on by the mineral water of the cave. I will here add that on the testimony of travelers no Negritos were on Samar. The island lies in the neighborhood of the Visayas. Although no description of the position of the skull is at hand and of the skeleton to which it apparently belonged, it

* NOTE.—In the matter of evidence for high antiquity and separate race furnished by incrusted cave crania, Prof. William H. Holmes's paper on the Calaveras skull (printed in this volume), should be studied, in which serious doubts are thrown upon the value of such relics as witnesses.—TRANSLATOR.

† F. Jagor, Grabstätten zu Nipa-Nipa. Zeitschrift für Ethnologie, 1869, I, p. 80.

‡ Die Philippinen und ihre Bewohner. Verh. der Berliner Anthrop. Gesellsch., 1870, session of 25th of January.

must be assumed that the dead man was not laid away in a coffin, but placed on the ground; that, in fact, he belonged to an earlier 'period.' How long ago that was can not be known, unfortunately, since no data are at hand; however, the bones are in a nearly fossilized condition, which allows the conclusion that they were deposited long ago.

The deformation itself furnishes no clue to a chronological conclusion. In Thévenot* is found the statement that, according to the account of a priest, probably in the 16th century, the custom prevails in some of the islands to press the heads of new-born babes between two boards, also to flatten the forehead, 'since they believed that this form was a special mark of beauty.' A similar deformation, with more pronounced flattening and backward pressure of the forehead, is shown on the crania which Jagor produced from a cave at Caramuan in Luzon. There are modes of flattening which remind one of Peru. When they came into our hands it was indeed an immense surprise, since no knowledge of such deformation in the South Sea was at hand. First our information led to more thorough investigations; so we are aware of several examples of it from Indonesia and, indeed, from the South Sea (Mallicolo). However, this deformation furnishes no clue to the antiquity of the graves.†

I have sawed one of these skulls in two along the sagittal suture. The specimen gives a good idea of the amount of compression and of the violence which this skull endured when quite young. The cranial cavity is inclined backward and lengthened, and curves out above, while the occiput is pressed downward and the region of the front fontanelle is correspondingly lacking. Likewise, a considerable thickness of the bone is to be noted, especially of the vertex. The upper jaw is slightly prognathous and the roof of the mouth unusually arched.

For the purpose of the present study, it is unnecessary to go further into particulars. It might be mentioned that all Lanang skulls are characterized by their size and the firmness of bone, so that they depart widely from the characteristics of the other Philippine examples known to me. Similar skulls have been received only from caves, which exist in one of the little rocky islands east from Luzon. They suggest most Kanaka crania from Hawaii, and Maiori crania from Chatham islands, and they raise the question whether they do not belong to a migration period long before the time of the Malays. I

* Rélations des diverses voyages curieux. Paris, 1591 (1663).

[† Chinese and Korean pottery are said to have been found with the deformed crania. Similar deformations exist in the Celebes, New Britain, etc. Head-shaping has been universal, cf. A. B. Meyer, Uber kunstliche deformirte Schädel von Borneo und Mindanao and über die Verbreitung der Sitte der kunstlichen Schädeldeformirung, 1881, 36 pp., 4°.—Translator.]

have, on various occasions, mentioned this probable pre-Malayan, or at least proto-Malayan, population which stands in nearest relation to the settling of Polynesia. Here I will merely mention that the Polynesian sagas bring the progenitor from the west, and that the passage between Halmahera (Gilolo) and the Philippines is pointed out as the course of invasion.

At any rate, it is quite probable that the skulls from Lanang, Cragaray and other Philippine islands are the remains of a very old, if not autochthonous, prehistoric layer of population. The present mountain tribes have furnished no close analogies. As to the Igorrotes, which Blumentritt attributes to the first invasion, I refer to my description* given on the ground of chronological investigations; according to the account given by Hans Meyer† the disposal of the dead in log coffins and in caves still goes on. Of the skulls themselves, none were brachycephalous; on the contrary, they exhibit platyrrhine and in part decidedly pithecoid noses. On the whole, I came to the conclusion, as did earlier Quatrefages and Hamy, that 'they stand next in comparison with the Dayaks of Borneo,' but I hold yet the impression that they belong to a very old, probably pre-Malay, immigration.‡

* Schädel der Igorroten. Verhl. der Berliner Anthrop. Gesellsch., 1883, pp. 390, 399. [On the Igorrotes see A. B. Meyer, Negritos, 1899, p. 12, note 2. —TRANSLATOR.]

† Die Igorroten von Luzon, p. 386.

‡ With this study of crania should be read Dr. A. B. Meyer, on craniological data and their value, in The Distribution of the Negritos, Dresden, 1899, in which he says: "The form of the skull in general is variable and can not be regarded as a permanent character in the development of the races." The reader must not neglect Dr. Meyer's publications, since in them he has the results of careful studies on the spot: Volume VIII, of the folio publications of the Dresden Royal Ethnographic Museum, 1890, on the tribes of Northern Luzon; Volume IX, of the same, on the Negritos, 1893; Album of Philippine Types, 1885, 32 plates, 4°; ditto, 1891, 50 plates; and The Distribution of the Negritos in the Philippine Islands and Elsewhere, Dresden. The last three are published by Stengel & Co., Dresden. The little book on distribution is in English, and contains, in addition to most useful information, a list of Blumentritt's publications.—TRANSLATOR.

A STUDY OF BRITISH GENIUS.

BY HAVELOCK ELLIS.

VIII.—PATHOLOGY.

IN a large proportion of cases no reference is made by the national biographers to the diseases from which their subjects suffered, nor to the general state of health. This, however, we could scarcely expect to find, except in those cases in which the state of health had an obvious influence on the life and work of the eminent person. In most of these exceptional cases it is probable that the biographers have duly called attention to the facts, and though the information thus attained is not always precise—in part owing to the imperfection of the knowledge transmitted, in part to the medical ignorance of the biographers, and in part to the deliberate vagueness of their reference to 'a painful malady,' etc.—it enables us to reach some very instructive conclusions concerning the pathological conditions to which men of genius are most liable.

Putting aside the cases of delicate health in childhood, with which I have already dealt in a previous section, the national biographers state the cause of death, or mention serious diseased conditions during life, in 322 cases.

It is natural to find that certain diseased conditions which are very common among the ordinary population are also very common among men of preeminent intellectual ability. Thus, a lesion of the vessels in the brain (the condition commonly described as paralysis, apoplexy, effusion on the brain, etc.) is a very common cause of death among the general population, and we also find that it is mentioned thirty-five times by the national biographers. Consumption, also, so prevalent among the general population, occurred in at least thirty cases. While many of the consumptive men of genius lived to past middle age, or even reached a fairly advanced age, the disease is responsible for the early death of most of the more eminent of those men of genius who died young—of Keats in poetry, of Bonington and Girtin in art, of Purcell (probably) in music. Some appear to have struggled with consumptive tendencies during a fairly long life; these have usually been men of letters, and have sometimes shown a feverish literary activity, their intellectual output being perhaps more remarkable for quantity than quality. But Sterne in literature, and Black, Priestley, Clifford and other eminent men of science are to be found among the consumptives. It is evident that the disease by no means stands in the way of all but the very highest intellectual attainments, even if it is not indeed actually favorable to mental activity.

Other forms of lung diseases are only mentioned fifteen times. The striking point here is the remarkable frequency of asthma in so small a group. It occurs nine times. It is fairly evident that in nearly all these cases we are concerned with true spasmodic asthma, a malady of the nervous system, and apt to arise, often in early life, on the basis of a somewhat neurotic organism.

Another malady to which we may judge that men of intellectual eminence are specially liable, since it is so often referred to, is angina pectoris. Heart disease—doubtless because its exact diagnosis is of comparatively recent date—is only referred to eighteen times, but in as many as eight or nine of these cases the disease is either distinctly stated to be, or may reasonably be inferred to be, angina pectoris. None of these cases are purely literary men, but four of them are artists.

There is, however, a pathological condition which occurs so often, in such extreme forms, and in men of such preeminent intellectual ability, that it is impossible not to regard it as having a real association with such ability. I refer to gout. This is by no means a common disease, at all events at the present day. In ordinary English medical practice at the present day, it may safely be said that cases of gout seldom form more than one per cent. of the chronic disorders met with. Yet gout is of all diseases that most commonly mentioned by the national biographers; it is noted as occurring in thirty-eight cases, often in very severe forms. We have, indeed, to bear in mind that gout has been recognized for a very long time, and that it is moreover a disease of good reputation. Yet, even if we assume that it has been noted in every case in which it occurs among our 902 eminent persons (an altogether absurd assumption to make), we should still have to recognize that it occurs in over four per cent. Moreover, the eminence of these gouty subjects is as notable as their number. They include Milton, Harvey, Sydenham, Newton, Gibbon, Fielding, Johnson, Wesley, Landor, W. R. Hamilton and Darwin, while Bacon was of gouty heredity.* It would probably be impossible to match the group of gouty men of genius, for varied and preeminent intellectual ability, by any combination of non-gouty individuals on our list. It may be added that these gouty men of genius have frequently been eccentric, often very irascible—'choleric' is the term applied by their contemporaries—

* Sydenham, the greatest of English physicians, who suffered from gout for thirty-four years, and wrote an unsurpassed description of its symptoms, said in his treatise, 'De Podagra,' that "it may be some consolation to those sufferers from the disease who, like myself and others, are only modestly endowed with fortune and intellectual gifts, to know that great kings, princes, generals, admirals, philosophers and many more of like eminence have suffered from the same complaint, and ultimately died of it. In a word, gout, unlike any other disease, kills more rich men than poor, more wise than simple." And another ancient (Father Balde) called gout *Dominus morborum et morbus dominorum.*

and occasionally insane. As a group, they are certainly very unlike the group of eminent consumptives. These latter, with their febrile activities, their restless versatility, their quick sensitiveness to impressions, often appear the very type of genius, but it is a somewhat feminine order of genius. The genius of the gouty group is emphatically masculine, profoundly original; these men show a massive and patient energy which proceeds not only 'without rest,' but 'without haste,' until it has dominated its task and solved its problem.

This association of genius and gout cannot be a fortuitous coincidence. The secret of the association probably lies in the special pathological peculiarities of gout. It is liable to occur in robust, well-nourished individuals. It acts in such a way that the poison is sometimes in the blood, and sometimes in the joints. Thus not only is the poison itself probably an irritant and stimulant to the nervous system, but even its fluctuations may be mentally beneficial. When it is in the victim's blood his brain becomes abnormally overclouded; when it is in his joints his mind becomes abnormally clear and vigorous. There is thus a well-marked mental periodicity; the man liable to attacks of gout is able to view the world from two entirely different points of view; he has, as it were, two brains at his disposal; in the transition from one state to another he is constantly receiving new inspirations, and constantly forced to gloomy and severe self-criticism. His mind thus attains a greater mental vigor and acuteness than the more equable mind of the non-gouty subject, though the latter is doubtless much more useful for the ordinary purposes of life.

It must not be supposed that in thus stating a connection between gout and genius it is thereby assumed that the latter is in any sense a product of the former. All the uric acid in the world will never suffice of itself to produce genius, and it is easy enough to find severe gout in individuals who are neither rich nor wise, but merely hard-working manual laborers of the most ordinary intelligence. It may well be, however, that, given a highly endowed and robust organism, the gouty poison acts as a real stimulus to intellectual energy and a real aid to intellectual achievement. Gout is thus merely one of perhaps many exciting causes acting on a fundamental predisposition. If the man of genius is all the better for a slight ferment of disease, we must not forget that if he is to accomplish much hard work he also requires a robust constitution.

It may be added that the other diseases of the uric acid group are common among our men of genius. Rheumatism, indeed, is not mentioned a very large number of times, considering its prevalence among the ordinary population. But stone, and closely allied conditions, are mentioned seventeen times (five times in association with gout), and as we may be quite sure that this is a very decided underestimate, we must certainly conclude that the condition has been remarkably common.

One other grave pathological state remains to be noticed in this connection—insanity. To the relationship of insanity with genius great importance has by some writers been attached. That such a relationship is apt to occur cannot be doubted, but it is far from being either so frequent or so significant as is assumed by some writers, who rake together cases of insane men of genius without considering what proportion they bear to sane men of genius, nor what relation their insanity bears to their genius. The interest felt in this question is so general that we may be fairly certain that the national biographers have rarely failed to record the facts bearing on it, although in some cases these facts are dubious and obscure. They may often have passed over gout without mention, but they have seldom failed to mention insanity whenever they knew of its occurrence. It is, therefore, possible to ascertain the prevalence of insanity among the persons on our list with a fair degree of approximation to the truth as it was known to the eminent man's contemporaries. We thus find that twenty-one were certainly insane at some period during the prime of their lives; that thirteen others were probably, but not certainly, insane at some period earlier than old age, and that in eleven further cases mental decay set in before death took place in old age. It may be added that at least nine committed suicide, and that at least fifteen were to a very high degree eccentric, although there is no clear reason to suppose that they were actually insane. It also appears that in seven cases (two fathers and five mothers) one of the parents became insane, and that in eight cases one or more of the children were insane. So that the insanity of the ascendants and descendants, so far as can be seen, was about equal and by no means excessive. If we include every possible case of insanity which may be inferred from the data supplied by the national biographers, and even if we include that decay of the mental faculties which is naturally liable to occur before death in extreme old age, we find that the ascertainable incidence of insanity among our 902 eminent persons is nearly 5 per cent.

It is certainly a high proportion. I do not know what is the number of cases among persons of the educated classes living to a high average age in which it can be said that insanity has occurred at least once during life. It is doubtless lower, but at the same time it can scarcely be so very much lower that we are entitled to say that there is a special and peculiar connection between genius and insanity. The association of genius with insanity is not, I believe, without significance, but in face of the fact that its occurrence is only demonstrable in 5 per cent. cases, and that it is only in 1 per cent. cases demonstrable in the parents puts out of court any theory as to genius being a form of insanity.

While I cannot compare with any precision the liability of these

persons of genius to insanity with the similar liability of corresponding normal classes, there is one comparison which it is interesting to make. We may compare the liability of persons of genius to insanity with the similar liability of their wives or husbands. It is noted by the national biographers that in fourteen cases the wives or husband (there is only one case of the latter) became insane. We may be fairly certain that this is a decided underestimate, for while the biographers would hold themselves bound to report the insanity of their subjects, they would not consider themselves equally bound to give similar information concerning the wives, while in other cases it may well be that the record of the fact has been lost. If now, in order to make the comparison reasonably fair, we omit the cases of senile decay, and only admit two-thirds of the doubtful cases of insanity, we find that the proportion of cases of insanity among the persons of genius is 3.3 per cent. Among the conjugal partners, on the other hand (I have not made any allowance for second marriages), it is 2.4. Thus we see that on a roughly fair estimate the difference between the incidence of insanity on British persons of genius and on their wives or husbands is less than one per cent. When we bear in mind that the data on which one of our groups is based are much more complete than those on which the other is based, it is not hazardous to assert that British men of genius have probably not been more liable to insanity than their wives.

At the first glance it might seem that this may be taken to indicate that the liability of genius to insanity is exactly the normal liability. That, however, would be a very rash conclusion. If the wives of men of genius were chosen at random from the general population it would hold good. But there is a well-recognized tendency—observed among all the mentally abnormal classes—for abnormal persons to be sexually attracted to each other. That this tendency prevails largely among persons of eminent intellectual ability many of us may have had occasion to observe. What we see, therefore, is not so much the conjunction of an abnormal and a normal class of persons, but the presence of two abnormal classes.

With regard to the significance of insanity, it must be pointed out that, although there may be an unusual liability to insanity among men of genius, there is no general tendency for genius and insanity, even when occurring in the same individual, to be concomitant. Just as it is rare to find anything truly resembling genius in an asylum, so it is rare to find any true insanity in a man of genius when engaged on his best work. The simulation of it may occur—the 'divine mania' of the artistic creator, or a very high degree of eccentricity—but not true and definite insanity. There seem to be only two certain (and two or three possible) cases—mostly poets—in which the best work was done during the actual period of insanity. Periods of insanity may alternate with

periods of high intellectual achievement, just as gout may alternate with various neurotic conditions, but the two states are not concomitant, and genius cannot be accurately defined as a disease.

It must also be pointed out, in estimating the significance of the relationship between genius and insanity, that the insane group is on the whole not one of commanding intellectual preeminence. It cannot compare in this respect with the gouty group, which is about the same size, and the individuals of greatest eminence are contained in the 'probable and doubtful' sections of the insane group. Among poets and men of letters, of an order below the highest, insanity has been somewhat apt to occur; it has been especially prevalent among antiquarians, but the intellectual eminence of antiquarians is often so dubious that the question of their inclusion in my list has been a frequent source of embarrassment.

If we turn from insanity to other grave nervous diseases, we are struck by their rarity. It is true that many serious nervous diseases have only been accurately distinguished during the past century, and that we could not expect to find much trace of them in the dictionary. But that cannot be said of epilepsy, which has always been recognized, and in a well-developed form cannot easily be ignored. Yet epilepsy or an epileptoid affection is only mentioned twice by the national biographers—once as occurring in early life (Lord Herbert of Cherbury), once in old age (Sir W. R. Hamilton), never during the working life. Although some of the most famous men in the world's history have been epileptics, it cannot be said that the lives of British men of genius favor the belief in any connection between genius and epilepsy, nor, so far as can be seen, do they furnish a single shred of evidence in support of the theory that genius is an epileptoid neurosis.

While, however, grave nervous diseases of definite type seem to be rare rather than common among the eminent persons with whom we are dealing, there is ample evidence to show that nervous symptoms of vaguer and more atypical character are extremely common. The prevalence of eccentricity I have already mentioned. That irritable condition of the nervous system which, in its Protean forms, is now commonly called neurasthenia, is evidently very widespread among them, and probably a large majority have been subject to it. Various definite forms of minor nervous derangement are also common, especially stammering or stuttering; this is noted as occurring in nine cases.* In seventeen other cases we are told that the voice was shrill,

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* Even this means a higher proportion than is found among the general population, and it must be remembered that the real occurrence must be reckoned as at least double that which may be ascertained from the 'Dictionary.' The normal occurrence of stuttering and stammering among adults is much below one per cent., and even among children it is under one per cent.

weak or small. Short-sight, another condition occurring on a basis of hereditary nervous defect, is noted as occurring in an extreme degree thirteen times; and in a certain number of cases the other senses are defective or absent. Convulsive or twitching movements of the face, etc., are unusually frequent, and are noted in nine cases.

A condition to which I am inclined to attribute considerable significance from the present point of view is clumsiness in the use of the hands and awkwardness in walking. A singular degree of clumsiness or awkwardness is noted many times by the national biographers, although they have certainly regarded it merely as a curious trait, and can scarcely have realized its profound significance as an index to the unbalanced make-up of the nervous system. This peculiarity is very frequently noted as occurring in persons who are tall, healthy, robust, full of energy. As boys they are sometimes not attracted to games, and cannot, if they try, succeed in acquiring skill in games; as they grow up all sorts of physical exercise present unusual difficulties to them; they cannot, for instance, learn to ride; even if fond of shooting, they may be unable to hit anything; they cannot write legibly; in walking they totter and shuffle unsteadily; they are always meeting with accidents. Priestley, though great in experiment, was too awkward to handle a tool; Macaulay could not wield a razor or even tie his own neckcloth; Shelley, though lithe and active, was always tumbling upstairs or tripping on smooth lawns. It would be easy to fill many pages with similar examples. It is noted of thirty-four eminent men on our list that they displayed one or more such inaptitudes to acquire properly the muscular coordinations needed for various simple actions of life. In numerous cases this clumsiness was combined with voice defect.

The existence of all these nervous incoordinations and defects is not evidence of disease, but it is yet in harmony with the evidence that we have obtained regarding the diseases most prevalent among British persons of genius. We have seen that the national biographers have revealed the special frequency of consumption, of spasmodic asthma, of angina pectoris, of gout, among persons of high intellectual aptitude. To a large extent these pathological conditions are closely related, and even interchangeable among themselves; they are all closely related to various neurotic conditions. A man of genius may indeed be, as it were, highly charged with nervous energy, but that energy is apt to be ill-balanced, and by no means always equably and harmoniously distributed throughout the organism.

THE INTELLIGENCE OF MONKEYS.

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A GOOD test of the intelligence of any animal is its ability to learn to do a thing by being shown it or by being put through the requisite movements. Human adults would learn readily in either of these ways, because we thus get ideas of what to do and how to do it and modify our actions in accordance with these ideas. If the reader had never seen a glass or a faucet, he would nevertheless learn how to get a drink by turning a faucet and holding the glass beneath it, if he saw some one else do it, or if some one took his hands and put them through the movements. The intelligence required in such cases is not of a very advanced sort; it is not the power of abstract reasoning or of seeing the relationships of facts, but is simply the capacity to have ideas and to progress from the idea of doing a thing to the act itself.

A study which I made four years ago of the mental powers of dogs, cats and chicks showed that these animals did not, at least not habitually, learn from this sort of tuition. They learned only in the following manner: If in any situation their own impulses led them to do something which brought desirable results, they would, when put in that situation again, do that particular thing rather than anything else. If for instance a kitten was shut up in a box from which it could escape only by turning around a button which held the door, it would claw and bite and pull and squeeze at random as its instinctive impulses led it to do. If by chance it made a pull at the button and so secured freedom and food, it would, the next time it was put in that box, be likely to make that particular movement earlier than in its first trial. After enough trials it would pull the button around as soon as put into the box. It had learned by the selection of one of its own impulses. If you showed it how to get out by putting it in the box and turning the button for it, thus letting it out, it learned no more quickly than when by itself. So also if you took its paw and with it pulled the button round. And any acts which it failed to learn by means of the selection of chance successes from its own impulsive activities, it could never be taught by example or by being put through the movements, scores of times.

During 1900 I was engaged in investigating the mental capacities of monkeys and included in my experiments a number bearing upon this question. The monkeys are quicker to learn and learn more

things than do the dogs and cats. Considering this and also their close relationship physically to man, it seemed of special interest to discover whether they could learn from example or from being put through certain movements, whether they would manifest the capacity so evident in man and so lacking in the lower animals in general.

I had one monkey for over a year and two others for about four months. All three were of the genus Cebus. The general method of the experiments was as follows: Boxes about a foot square were made with small doors held by all sorts of contrivances. For instance, one was held by a hook, another by a bar, another by a bolt, another by a wire fastened firmly at one side and wound round a nail at the other. A bit of food was put inside such a box and the door of the box left open. The box was then put inside one of the large cages containing a monkey. It would come down, reach into the box and get the food. After this had been repeated a few times the box would be put into the cage, with its door shut. The monkey would try to get in as before. He might chance to operate the simple mechanism that held the door. If, however, he did not succeed of his own impulsive activity I would show him or put him through the movement a few times and then leave him to himself again to see if he had profited by the tuition.

The general result was that they did not profit by the tuition, that they did not gain and use ideas of how to open the doors, but learned only by a process of selection from their own impulses. The meaning and value of this general fact will appear in the details of the experiments.

In order that such experiments shall be valid tests of the workings of an animal's mind it is necessary that he surely desire to get into the box, that he be not disturbed by the surroundings in any way that will alter his mental efficiency, and that the experimenter be able to handle him easily without frightening him or taking his attention away from the box. In all cases it is further necessary to make sure that the monkey sees you perform the acts you expect him to imitate, and sees and feels himself make the movements you put him through. These desiderata were obtained by testing the monkeys when hungry and using bits of food of which they were especially fond as the attraction; by experimenting with them after they were quite used to their habitat and to my presence; by getting them into the habit of coming to me and enjoying being handled, having their paws taken, etc.; by showing them the act or putting them through it only when they were attending to the box.

A sample of one of the experiments on the influence of example is the following: The box used was arranged so that the door opened when a brass lever was depressed about an eighth of an inch. The monkey could reach this lever by putting his hand through a hole about an inch

square at the right side of the front of the box. The door was at the left side of the front. On January 12th I put this box in No. 3's cage, the door of the box being open. I put a bit of food in the box. No. 3 reached in and took it. This was repeated three times. I then put in a bit of food and closed the door. No. 3 pulled and bit the box, turned it over, fingered and bit at the hole where the lever was, but did not succeed in getting the door open. After ten minutes I took the box out. The monkey having failed by his own impulsive efforts to depress the lever, I began the tuition. I took No. 3 out and let him sit on my knees (I sitting on the floor with the box in front of us). I would then put my hand out toward the box and when he was looking at it would insert my finger and depress the lever with as evident a movement as I could. The door, of course, opened, and No. 3 put his arm in and took the bit of food. I then put in another, closed the door and depressed the lever as before. No. 3 watched my hand pretty constantly, as all his experiences with me had made such watching profitable. After ten such trials he was put back in the cage and the box put in with a large piece of food in it and its door closed. No. 3 failed in the course of five minutes to get the door open. His behavior was just the same as it had been before he had seen me open the door ten times. He had not profited at all by my example. Later I showed him 15 times more and then tried him by himself. He failed as before.

The two monkeys, No. 1 and No. 3, were given a number of such chances to learn acts from seeing me. Other boxes were used, the doors of which could be opened by pulling up a bolt, pulling out a plug, pushing a bar back into a slot, unwinding a wire and pulling a loop off from a nail. I had also certain pieces of apparatus arranged which would throw a bit of food down a chute into the cage when some simple mechanisms were operated; when for instance a nail was pulled out of a hole or a loop pulled off a nail or a bar pushed in. These could be set up outside the cages so that the monkeys could reach them through the wire netting and could easily see me operate them.

No. 1 had in all eight chances to learn from seeing me. In seven of the cases he failed utterly after seeing me operate the mechanisms 21, 5, 10, 4, 15, 40 and 15 times respectively. He did succeed in one case where the act required was to pull a wire loop off a nail. This must, I think, have been an accident. The other monkey failed utterly to learn to do the same thing though he had continued tuition.

No. 3 had seven chances to learn from seeing me. In five out of the seven he failed after seeing me operate the mechanisms 40, 30, 25, 5 and 30 times respectively. In the case of the other two, although he succeeded in getting the door open, it was not by doing as I had shown him. I opened a door 25 times by pulling a bolt up, but he opened it by pulling and pushing at the door itself until he worked the bolt up out

of place. In the other case I pulled a hook out from a catch but he yanked at the bar to which the hook was attached and so jerked the latter free.

It might be that although the monkeys did not succeed after tuition where they had previously failed, yet they *attempted* acts which they had not previously attempted. This is not the case, however. There were no signs that the monkeys tried more after tuition to do the things they saw me do than they did before. Their behavior was unmodified by the tuition save that in general they tried less.

It may be objected that the acts I failed to teach the monkeys were not consistent with their make-up, that a monkey might be very intelligent and still not manifest his intelligence by depressing levers, unwinding wires or pulling off loops, that monkeys might be able to learn to do certain things from seeing them done and still be unable to learn the particular acts needed in these experiments. But as a matter of fact, these particular acts were quite natural for the monkeys, quite in accord with their interests and propensities. They learned by the typical animal method acts of the same general sort, *e. g.*, to open boxes and operate the mechanisms throwing food into their cages by pulling bars around, unhooking hooks and pulling at strings. And often the very same act with which I tested one monkey in the experiments just described had been learned by another through the repetition and selection of a chance success. Thus No. 1 learned of himself to unwind a wire though No. 3 failed to do so after seeing me do it 30 times.

The systematic experiments designed to detect the presence of ability to learn from human beings are thus practically unanimous against it. So too was the general behavior of the monkeys, though I do not consider the failure of the animals to imitate common human acts as of much importance save as a rebuke to the story-tellers and casual observers. The following facts are samples: The door of No. 1's cage was closed by an iron hoop with a slit in it through which a staple passed, the door being held by a stick of wood thrust through the staple. No. 1 saw me open the door of his and other cages by taking out sticks hundreds of times, but though he escaped from his cage a dozen times in other ways he never took the stick out and to my knowledge never tried to. I myself and visitors smoked a good deal in the monkeys' presence but a cigar given to them was always treated like anything else.

The following is a sample of the tests of the monkey's ability to learn to do a thing from being made to do it: A box was arranged with its door held closed by a bar of wood held in position in a slot. When it was pushed back an inch and a half further into this slot the door could be opened. It was fastened so that it could not be pulled out from the slot altogether. The only way to get the door open was

thus to push or pull the bar back. It worked very easily, a pressure of perhaps 15 grams being sufficient. On January 4, 1901, this box was put in No. 1's cage. He failed to get in in 5 minutes, though he was active in trying to get in for about 4 minutes of the time and pulled and pushed the bar a great deal, though up and down and out instead of back. In his aimless pushings and pullings he nearly succeeded. He failed in 5 minutes in a second trial also. I then opened the door of the cage, sat down beside it, held out my hand, and when he came to me took his right paw and with it (he being held in front of the box) pushed the bar back (and pulled the door open in those cases when it did not fall open of itself). He reached in and took the food and went back to the top of his cage and ate it. I put him through the act thus 10 times. I then let him try alone. He failed to get in. In this and the two following days No. 1 was put through the act 80 times and given frequent opportunities to open the box himself. He never derived the slightest profit from the tuition.

No. 1 had eight such tests and No. 3 had six. Their behavior was in some cases ambiguous but the verdict would surely be that they had no general capacity to acquire these simple habits by seeing and feeling themselves make the movements and get food thereby.

The theoretical importance of the failure of the monkeys to learn from example or from being put through movements consists in the testimony it bears to their lack of a general fund of ideas. Adult human beings learn to do things by getting ideas of the circumstances and of the acts required and then proceeding to act upon these ideas. We think of where we are going, and so go; we have an idea of what we wish to do and so do it. Rarely if ever do monkeys learn in this way.

The behavior of the monkeys apart from these specific experiments seemed also to show their inability to acquire and use ideas of objects or acts. In getting them so that they would let themselves be handled, it was of almost no service to *take* them and feed them while holding them or otherwise make that state pleasant for them. By far the best way is to wait patiently till they do come near, then feed them; wait patiently till they do take hold of your arm, then feed them. If you do take them and hold them partly by force you must feed them only when they are comparatively still. In short in taming them one comes unconsciously to adopt the method of rewarding certain of their impulses rather than certain *conditions* which might be associated in their minds with ideas, had they such.

Monkey No. 1 apparently enjoyed scratching himself. Among the stimuli which served to set off this act of scratching was the irritation from tobacco smoke. If anyone blew smoke in No. 1's face he would blink his eyes and scratch himself, principally in the back. After a time he got in the habit of coming to the front of his cage when anyone

was smoking and making such movements and sounds as in his experience had attracted attention and caused the smoker to blow in his face. He was often given a lighted cigar or cigarette to test him for imitation. He formed the habit of rubbing it on his back. After doing so he would scratch himself with great vigor and zest. He came to do this always when the proper object was given him. I have recounted all this to show that the monkey enjoyed scratching himself. *Yet he apparently never scratched himself except in response to some sensory stimulus.* He did not with all his experiences of scratching ever get the idea of that act and use it to arouse the delightful act. He was apparently incapable of thinking 'scratch' and so doing. Yet the act was quite capable of association with circumstances with which as a matter of hereditary organization it had no connection. For by taking a certain well-defined position in front of his cage and feeding him whenever he did scratch himself I got him to scratch always within a few seconds after I took that position.

The fact that monkeys do not possess the human type of ideas must not be taken as evidence that they are no nearer relatives to us mentally than are the other lower animals. On the contrary they occupy an intermediate position in every main psychological feature between mammals in general and the human species.

The essentials in an inventory of an animal's mental capacities are its sense powers, the kinds of movements it can make and their delicacy, complexity and number, its instincts or the sum of those tendencies to feel and act which it has apart from experience or learning, and its methods of learning or of modifying its behavior to suit the multitudinous circumstances of life. In each of these respects the monkeys show kinship with man.

In point of sense powers they rely little on smell and much on vision. They possess the power of clear, detailed vision which is absent, for instance, in dogs and cats and is so important a possession of man. A monkey will notice a hair on your hand or a pin six feet off. He thus resembles man in what has been universally recognized as the most intellectual of the senses.

In their motor equipment monkeys possess first of all the muscular coordinations necessary to sustain an upright position and consequently the free use of the fore-limbs. The movements of these fore-limbs are more in number and suited to more complex and varied tasks than are those of lower animals. The attractiveness of the monkey cage in a zoological garden is largely due to the similarity of the monkeys' movements and our own. The monkey not only has a body like a man's, but he also uses it like a man.

Our native tendencies are so metamorphosed by the education of a civilized environment that in adult age they seldom appear in recogniz-

able form. But if we take human beings at from 6 months to 3 years of age or later, we find plenty of traits that appear in the monkeys. In fact the human instinct which is perhaps of prime importance in human mentality, the instinct which perhaps is the real cause of many of our most boasted powers, has its clear prototype and homologue in the monkey. I refer to the instinctive enjoyment of physical and mental activity in general, to the tendencies to act and feel as much as possible, regardless of any ulterior practical considerations, which we sometimes call destructiveness or constructiveness and curiosity.

Even the casual observer, if he has any psychological insight, will be struck by the general, aimless, intrinsically valuable (to the animal's feelings) physical activities of a monkey compared with the specialized, definitely aroused, utilitarian activities of a dog or cat. Watch the latter and he does but few things, does them in response to obvious sense presentations, does them with practical consequences of food, sex-indulgence, preparation for adult battles, etc. If nothing that appeals to his special organization comes up, he does nothing. Watch a monkey and you cannot enumerate the things he does, cannot discover the stimuli to which he reacts, cannot conceive the raison d'être of his pursuits. Everything appeals to him. He likes to be active for the sake of activity.

The observer who has proper opportunities and takes proper pains will find this intrinsic interest to hold true of mental activity as well. No. 1 happened to hit a projecting wire so as to make it vibrate. He repeated this act hundreds of times in the few days following. He could not eat, make love to or get preliminary practise for the serious battles of life out of that sound. But it did give him mental food, mental exercise. Monkeys seem to enjoy strange places; they revel, if I may be permitted an anthropomorphism, in novel objects. They like to have feelings as they do to make movements. The fact of mental life is to them its own reward.

Finally in their method of learning, although monkeys do not reach the human stage of a rich life of ideas, yet they carry the animal method of learning by the selection of impulses and association of them with different sense impressions, to a point beyond that reached by any other of the lower animals. In this, too,they resemble man; for he differs from the lower animals not only in the possession of a new sort of intelligence but also in the tremendous extension of that sort which he has in common with them. A fish learns slowly a few simple habits. Man learns quickly an infinitude of habits that may be highly complex. Dogs and cats learn more than the fish, while monkeys learn more than they. In the number of things he learns, the complex habits he can form, the variety of lines along which he can learn them, and in their permanence when once formed, the monkey justifies his inclusion with man in a separate mental genus.

COCAINE ANALGESIA OF THE SPINAL CORD.

BY SMITH ELY JELLIFFE, M.D., PH.D.

THERE are surgeons living to-day who remember the fascination and horrors of necessary operations, when speed was as great a requisite as skill to shorten the mortal agony, and when a famous surgeon would remove a limb in eleven minutes. There are many who remember how slowly the boon of chloroform worked its way against prejudice. To give it to ease the pain of childbirth was not only unsafe, according to the family doctor, but sacrilegious, according to the preacher, for did not the Holy Writ say, 'In sorrow shalt thou bring forth children,' and who of Adam's daughters should presume to escape the curse? Had it not been for the wit of Dr. Simpson, who insisted that the Lord performed the first surgical operation under anæsthesia when He caused Adam to fall into a deep sleep and took a rib from his side, and the courage of Queen Victoria, who set the example to the women of her empire by trusting her physician to give her chloroform at the birth of one of her children, it is quite possible that the ease from pain of all kinds might have been longer delayed.

Soon after chloroform came ether, the safer anæsthetic, and the one more frequently used, to produce unconsciousness in pain; and then cocaine, that peculiar drug that, injected into the tissues, benumbs the nerves and abolishes sensation of pain, and that gives the last word of the century on anæsthesia.

When the anæsthetic properties of this alkaloid of coca were discovered, and it had been demonstrated that abscesses could be opened and slight, but otherwise very painful, operations could be performed without pain, under its influence, it was considered the one thing necessary to complete the series of anæsthetics. The nerves, however, quickly recovered from the effects of the drug, and hence operations had to be accomplished in a comparatively short time. Until recently, only minor operations of the external parts of the body could be performed, and cocaine has been classed merely as a local anæsthetic; but its future has suddenly opened along new and startling lines in the discovery that when it is injected into the spinal cord it causes a total loss of sensation to pain below the point of puncture, so that most elaborate and difficult operations may be carried on while the patient chats pleasantly with the surgeon and attendants.

This discovery, like so many in medical science, did not flash into existence like a new star in the firmament, but was the result of

researches of different men of various nationalities, which finally culminated in a practical result.

It was to an American, a well-known physician in New York, that we owe the first suggestion of the idea. Dr. J. Leonard Corning, in 1885, discovered that frogs, those benefactors to the human race on whom so many of the experiments for the good of man have been tried, could get the characteristic reaction of strychnine from very small solutions of the drug if it were injected into the spinal cord. He then bethought him to try the effect of cocaine; he accordingly experimented on dogs, injecting the anæsthetic between the superior processes of the vertebræ, where the numerous minute vessels would carry it to the cord. After a few minutes the dog lost all sensation in its hind legs; it could be pinched and pricked and touched with an electric brush without knowing it, but the same treatment applied to its fore legs brought forth yelps and howls.

As there were no evil effects seen in the various dogs on which Dr. Corning experimented, he tried the effect upon one of his patients who had for some time suffered from spinal weakness; injecting sixty drops of a 3 per cent. solution of cocaine into the tissues about the spine, between the eleventh and twelfth dorsal vertebræ. For the space of half an hour the man had absolutely no sensations of pain in his lower limbs; electricity and pin pricks were alike unnoticed, and Dr. Corning might have amputated a leg had his patient needed to lose one of those members, and at one stroke have taken all the fame of the discovery of a new form of anæsthesia; but in an hour or more the patient arose and walked home, with no unpleasant after-effects, except for a slight headache and dizziness. Dr. Corning reported on the frog, and dog, and man, to his medical colleagues, and threw out a general hint as to the possibility of extending the usefulness of cocaine in operations; but there the matter dropped. His experiments were, however, the germ of a new idea.

Some years later, the German investigator, Quincke, devised a method of puncturing the membrane surrounding the cord, so that he might draw out a few drops of the spinal fluid, to see whether, in such a disease as spinal meningitis, for instance, there were any bacteria present, or whether he could discover any characteristics that would help to diagnose certain obscure cases of disease by observing the pressure of the fluid of the spinal cord.

To penetrate to the very marrow of one's backbone would have seemed, fifteen years ago, to toy with the seat of life. Dr. Corning did not go nearer than the nerve tissues near the cord; but, once Quincke had shown that the needle of a hypodermic syringe could be thrust easily and painlessly and accurately into the space surrounding the spinal cord, and that a few drops of the cerebro-spinal fluid might

be drawn out, it occurred to Dr. Bier, of Kiel, who was the true genius of the discovery, that a few drops of a cocaine solution might be put in, to produce local anæsthesia on a large scale. He worked out the technique of the injection, operated on conscious patients, and reported his success. Although the operations and experiments performed by Bier were published and commented on with interest, they aroused no special excitement beyond a small circle of investigators, and might have remained merely scientific experiments, had it not been for the International Medical Congress, which met at the Paris Exposition. The benefit of the interchange of thought of the ablest scientific men of all countries that is offered by these congresses, which have come into fashion in the last twenty-five years, is incalculable. Every medical journal in every language tells the physician and surgeon of something new, but every day's experience teaches him that it is better to pay attention to the workings of old laws, instead of trying to apply every new remedy; therefore, it was not surprising that even so great a discovery should meet tardy recognition, and should need the dramatic setting of a world exposition to place it prominently before the medical profession. This it obtained at the Paris clinics held by M. Tuffier, where, with all nations for eye-witnesses, he performed one operation after another on patients who were perfectly conscious and yet who were absolutely insensible to pain below the nipple line. His feats were the talk of the Congress; many of the most famous surgeons of the world were present and saw how comparatively simple it was, after first rendering the point of puncture insensible with a little cocaine, to cause the patient to lean forward, as if scorching on a bicycle, thus straining the vertebræ slightly apart, when it was easy to insert the hypodermic needle until the appearance of a few drops of the spinal fluid showed that the cord had been tapped, and then to attach the syringe and inject the cocaine solution.

The surgeons soon dispersed to their own parts of the planet, glorifying the deeds of Tuffier, almost forgetting Bier, who was Tuffier's authority, and never mentioning Corning, from whom came the original idea. But the question of homage was insignificant in comparison with the test of the idea, and, within a few months, the members of the Congress had, in their own clinics and practice, cocainized the spinal cord for laparotomies, amputations and childbearing, with a varying amount of success.

But, in spite of the fact that in some operations the patients left the operating table with almost no unpleasant sensations, yet in the majority there was dizziness, nausea and frightful headache, which in some cases lasted as long as eight days, in spite of everything to bring relief.

None of the surgeons who have attempted spinal cocainization seem to be able to agree upon the smallest quantity that will ensure anæsthesia, and they are between the horns of the dilemma, that too strong a solution produces violent poisonous effects on the body, and one too weak gives out before the operation is ended, causing the predicament in which one surgeon found himself, when the anæsthetic effects wore off when he was half through, and, having opened the abdomen, he did not dare to permit the patient to sit up and lean over for a second injection.

In all cases, surgeons feel safer to have chloroform or ether at hand in chance of failure, and, when all is said, they do not see any very great advantage in performing the operation under cocaine over the old method. Moreover, many of them say that there is something rather uncanny in the feeling that the patient is conscious of and perhaps watching every stroke of the knife, for, strange to say, sensations of heat and cold, touch and pressure, are still present, and only pain is absent. The older surgeons, before the days of any anæsthetic, mentioned this eerie feeling, and welcomed the patient's unconsciousness of what was being done to him as much as the patient did himself.

There is not enough yet known of the structure of that most wonderful detail of the human organism, the ganglionic nerve cell, to say what may be the effects of certain drugs upon it. Cocaine certainly has a sufficiently anæsthetic effect to make it valuable in those cases where an operation cannot be performed under ether or chloroform on account of a weak heart, or tendencies to asthma, kidney disease, or other complication. Where death would occur either with or without such an operation, it affords a comparatively safe loophole of escape; and at present it will perhaps be confined to such cases.

But, though cocaine anæsthesia cannot at present take the place of the other anæsthetics, it has given a hint of what may be developed; experiments will be continued, to render the lumbar puncture perfectly harmless, to determine the exact amount and strength of cocaine or any other drug that will produce a definite length of anæsthesia, to administer it in such a way as to lessen the unpleasant after-effects, and, if possible, to discover how the upper part of the body may also be rendered anæsthetic. The ideal, absolutely safe and universally applicable general anæsthetic is not yet discovered; but unquestionably a new way has been pointed out, and within a few years the results of scientific experiment will justify the hope that has sprung up at this method of cocainizing the spinal cord, the hope that anæsthesia is still in its infancy, and that when more is known about the effect of drugs on the nerve cells, it will be possible to banish pain, sensation and consciousness at will, and without danger to life.

THE EVIDENCE OF SNAILS ON CHANGES OF LAND AND SEA.

By HENRY A. PILSBRY, Sc.D.,

PHILADELPHIA ACADEMY OF NATURAL SCIENCES.

IF we wish to learn the history of any land area, we turn to its geology for a record of changes in the past. The time of its emergence from ocean, the age of its mountains and the details of its growth by successive increments of land elevated from the sea, all this we may expect to learn with reasonable accuracy, besides gaining a knowledge of the plants and animals which lived from time to time upon the coasts.

But we may push our inquiry beyond the shore, and ask, Over this expanse of sea did land once extend? Did an arm of the land reach to this island in the old time, or are the islands of that archipelago but the mountain tops of a sunken continent? To such questions geology gives no definite answer. In some cases, to be sure fjords tell their tale of sunken gorges, or soundings give evidence of a subsided coast, with river valleys and former coast-line indicated by submarine topography, as in the continuation of the Hudson River valley outward from New York Harbor, and the old shore-line, now at the hundred fathom contour. But these are exceptional cases; and the ocean bed, blanketed with modern deposits, usually gives but scant information to the geologist.

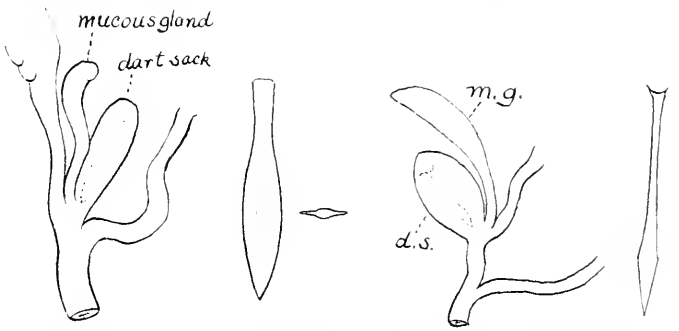
For the solution of the questions we must address ourselves to another and wholly different inquiry: the geographic distribution of living animals and plants.

To the pre-Darwinian naturalist, the relationships of animals among themselves and their distribution over the earth's surface were enigmas, quite insoluble upon the hypothesis of special creation. But the doctrine of descent, of the blood relationship of all the members of a genus and family, fills these problems with meaning. If we find that an island, such as England, has the same species of snails, earthworms, reptiles and fresh water crustacea and fishes as the neighboring continent, it becomes obvious that there has been a land connection in the past, for there is no other means by which any extensive fauna of these animals could have reached an island. If we take another island, and find that while it has different species from the mainland, yet they belong to the same genera, we must conclude that there has been actual land connection here also, though of more ancient date, across which the ancestors of these transformed species emigrated.

It is obvious that animals with feeble powers of travel are of the greatest value in these researches, because they indicate more ancient and more enduring changes of sea and land than freely mobile creatures, such as birds and quadrupeds, which may spread, conditions favoring, with great rapidity. Moreover, the invertebrates have changed much more slowly than higher animals; their evolution has been slow. Almost the whole great drama of mammalian evolution has been acted during Tertiary time, while there has scarcely been generic change in the mollusks! Mammals and birds reflect in their distribution the later earth movements, the invertebrates and fishes the earlier.

The Helix snails are particularly well adapted to show ancient faunal relationships, as they occur under one or another form in almost all lands. But it is only in the present decade that their anatomy has been understood, and the true relationships of the various groups of the family recognized. One of the most interesting developments of the anatomical study of land snails has been the demonstration of a close relationship existing between Helices of the Philippine Islands and eastern Asia and those of California, Mexico and the Greater Antilles.

Years ago Dr. Karl Semper, in his Travels in the Philippine Archipelago, showed that the arboreal Helices of the Philippines are provided with a muscular sack containing a calcareous needle—the so-called 'dart'—and surmounted by a mucous gland, the whole being



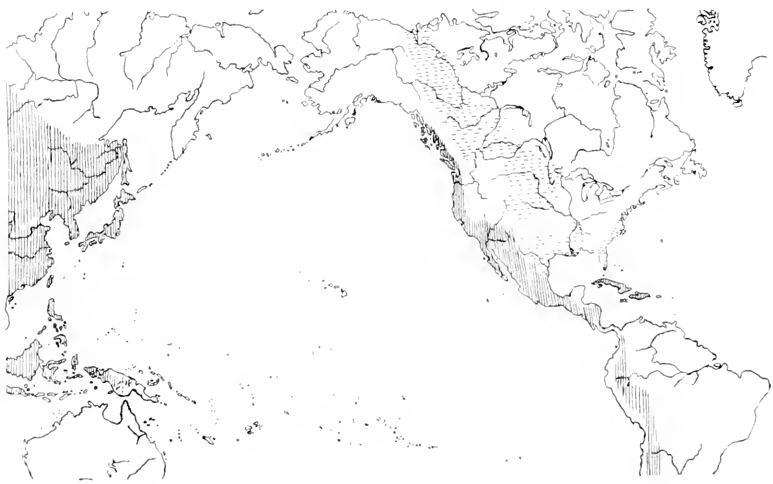
1. DART APPARATUS OF EPIPHRAGMOPHORA MORMONUM, A CALIFORNIAN SNAIL. 2. THE DART, ENLARGED. 3. DO. OF CHLORÆA BENGUETENSIS, PHILLIPPINES. THE POSITION OF THE DART IN ITS SACK IS SHOWN BY DOTTED LINES.

an appendage of the reproductive organs. In species of both China and Japan the same peculiarities are found in these organs. It was already known that the Helices of Europe have a similar sack and dart, but the associated mucous gland is split into finger-like tubes and removed from the sack to an adjacent duct. The function of the dart apparatus is not well understood. The snails thrust their darts into one another during the mating time, and hence the dart is believed to be an excitation organ.

Now, when the Helices of California, Mexico and a part of those of the West Indies were examined, it was found that they have *the dart apparatus, agreeing with species of Japan, China and the Philippines,* not with those of Europe or of eastern North America; for, to emphasize this resemblance, the Helices of the middle and eastern United States are anatomically totally unlike the Californian, Mexican and Antillean, having no dart-sack or mucous gland.

We are, therefore, confronted with a group of snails inhabiting both borders of the greatest ocean, but agreeing so closely in anatomy that no hypothesis but that of a common origin, descent from common ancestors, is conceivable. Our American dart-bearing Helices must surely look to distant shores in far-off times for their ancestry.

In the South the Oriental and Occidental members of the great group of dart-bearers are separated by the breadth of the Pacific, the islands of which are barren of related snails. In the North they are parted by many miles of barren coast; for in America the dart-bearers go no further north than Sitka, and in Asia they are not known much to the northward of the Japanese Empire.



MAP SHOWING DISTRIBUTION OF DART-BEARING HELICES IN VERTICAL LINES CRETACEOUS SEA IN BROKEN HORIZONTAL LINES. BEING ON MERCATOR'S PROJECTION, THE NORTHERN RANGES OF THE DART-BEARERS IN AMERICA AND ASIA APPEAR MUCH MORE SEPARATED THAN THEY REALLY ARE.

We know, however, that in Upper Cretaceous times the Arctic lands were not, as now, clad in the scanty green of mosses, lichens and herbs, with few deciduous trees, except stunted willows and the like, but they bore noble forests of magnolia, beech and birch, with red-woods and pines—such forests as snails love and thrive in, doubtless

with abundance of sheltering windfalls and rotting boles to give them refuge, and decaying leaves to feed the fungi and tender herbage, which are the food of snails.

It is not unlikely, then, that, in the distant past, when a kinder climate allowed the forests of the temperate zone to extend to the Arctic shores in Alaska and Siberia, the snails went with them; and, if we assume a very moderate elevation in the region of Bering Strait, connecting Alaska and Asia by a land bridge, there would be no bar whatever to the spread of forest trees and the emigration of snails from one continent to the other.

The distribution southward of the snails and other inhabitants of the forest would be merely a question of time in the absence of barriers in the form of lofty mountains, deserts or arms of the sea, running across their path.

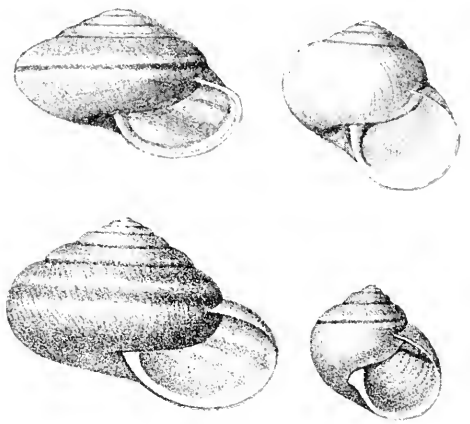
There are good reasons for believing that the dart-bearing snails originated in the Orient, and, if so, their migration was eastward to America. In all probability, the slowly upbuilding land mass in western America had none of the higher land snails before the advent of the Asiatic snails in the later Cretaceous, as it was profoundly isolated in earlier mesozoic and preceding time, so far as existing geological data show. On reaching America, the snails spread southward. Why, then, it may be asked, do we not have the descendants of the Asiatic dart-bearers in eastern North America? There are several reasons. During the Cretaceous period an inland sea extended from the Gulf of Mexico, through the Dakotas and northward to the Arctic Ocean, in the neighborhood of the Mackenzie River.* This would prevent the eastward spread of the snail emigrants from Asia. Since that time, the increasing height of the Rocky Mountains, and the arid conditions of much of the mountain region, with its poverty in deciduous trees, would be, and is to-day, an effectual bar to the eastward distribution of the Pacific slope snails.

In the Far West, however, no barriers prevented the southward spread of the dart-bearing Helices. They pushed south to Mexico, and, perhaps later, to the Andean region of South America. There was also undoubtedly a land bridge connecting an Antillean continent or archipelago with Central America, over which the dart-bearers passed to the Antilles. This connection is shown by many other groups of land snails also, the distribution of which can be explained in no other manner.

* Dawson maps the Cretaceous inland sea as extending to the Arctic Ocean. During the earlier Cretaceous it also reached the Pacific, though an archipelago probably extended north to Alaska; but in the Laramie there was a broad belt of land to the westward of the Cretaceous sea or lakes. See map, which represents the probable extent of the sea at the beginning of the Laramie.

The Helices of the West Indies lend no aid to those who advocate the hypothesis of an 'Atlantis' bridging or partially bridging the Atlantic, for they are not allied to species of Madeira, the Azores, Cape Verde or Canary Islands. Their anatomy is vastly nearer Mexican, Californian and East Asiatic groups.

No generalization based upon the distribution of snails, or of any one group of animals, can be satisfactory unless it is supported by the evidence of animals of other groups, and by that of plants. In extending the data relative to the zoogeography of America and Asia, it may be said that the evidence of the naked snails or slugs fully supports that of the Helices. The West American slugs have their cousins in China and the Himalayas, not in eastern North America. The fresh-water crayfish of our Pacific slope belong to the Old World genus *Astacus,* not to the East American genus *Cambarus.* The evidence of fishes seems to strongly favor a former connection of Asia and America. Thus, Gunther* deduces a Central Asian origin for the



Dart-Bearing Helices. Upper Figures Two Species of Eulota from Japan; Lower Figures Epiphragmophora from California.

Cyprinoids, or Carp family, which is also very numerously represented in America. Moreover, he regards the Chinese species of *Catostomus,* or 'sucker,' as a return emigrant from America to Asia. The North American catfishes belong to an East Asian group of the family; and our garpike has a representative in Chinese waters.

Such evidence as the higher vertebrates afford do not strengthen the case stated for the snails, because their evolution has been vastly more recent and rapid, and their means of distribution are far less restricted. Thus the horses have attained their present distribution since the Pliocene, but they are capable of spreading rapidly wherever

* The Study of Fishes, p. 244.

pasturage is to be found. The wide range of such groups as this, and the birds, is limited only by markedly unfavorable physical conditions, and their presence in both the Old and New Worlds merely indicates that up to quite recent times there has been a land bridge over Bering Strait.

The identical species of plants in Japan and the United States, elaborately discussed by Dr. Asa Gray, are also, in many cases, it would seem, comparatively recent emigrants into one continent or the other, not old enough to have become changed by new surroundings; or they are plants which lived in the Tertiary forests of Greenland and British America, which Heer and others have made known. Through stress of climate, this circumboreal flora has been driven southward, many of its species to be changed by the vicissitudes of the march, while others still flourish unchanged in the two continents.

THE BLUE HILL METEOROLOGICAL OBSERVATORY.

BY FRANK WALDO.

METEOROLOGY became established on an independent basis about fifty years ago. With the beginning of a systematic study of the atmospheric conditions there arose a demand for more frequent observations than could be made directly, and, as a result, self-registering meteorological instruments came into use. It was speedily found that the exceedingly sensitive and complicated apparatus necessary for furnishing accurate records of the atmospheric conditions required the services of thoroughly trained and skilful persons in its manipulation. Not only this, but proper exposure of the instruments and careful reduction of their records were necessary. In other words, the generally recognized requirements of a good astronomical observatory must be fulfilled in carrying on the work of a meteorological observatory.

It had long been supposed (and unfortunately is still by many) that any one is competent to make meteorological observations who is able to read a barometer scale or hold a measuring stick in a rainfall basin. The importance of having the instruments automatically record their indications became very generally recognized, if we may judge by the number of self-registering instruments constructed and set in operation, although the considerable cost prevented their general introduction. Then it was that the need for this work of well-trained observers began to be felt. Where meteorology was associated with one of the older physical sciences, such as astronomy, the necessary care was given to the meteorograph; but in most cases, after a brief and generally unsatisfactory trial, the self-recording instruments were kept going in only a perfunctory manner or allowed to fall entirely into disuse. While the necessity for meteorological observations continued, and continuous records became more and more imperatively demanded, yet it was not until the true conditions were fully realized and meteorological observatories comparable with those devoted to astronomical research were built, equipped and manned, that anything like satisfactory atmospheric observations were obtained. Nor was it longer deemed sufficient only to keep up the observation of the meteorological elements; the fact was emphasized that the results must be properly worked up and put into such a form as would best serve the purposes for which they were desired.

Observatories of various degrees of excellence and fitness were established at a number of places, but it was reserved for Professor Heinrich

Wild, then director of the Russian Meteorological Service (but now of Zurich), to set us a pattern of what a meteorological observatory should be, in the Pawlowsk Observatory near St. Petersburg.

It has been found by experience that the services of three thoroughly trained and skilled persons are necessary to properly conduct a meteorological observatory. It is the verbal testimony of Dr. Wild that it is better to do entirely without the records of self-registering instruments than to have the records made under the care of untrained and incompetent persons.

A. LAWRENCE ROTCH.

In the United States there long existed an apparent indifference to the demand for numerous continuous atmospheric records, the winds alone receiving the merited attention (except in an experimental way) from our Signal Service organization. A single exception to this indifference was the Central Park Observatory, which was operating unobtrusively along the right lines, but after the stereotyped manner of the older European observatories. For the rest we were mainly content with the observations made at fixed intervals during the day.

Such was the condition of observational meteorology in America at the time when Mr. A. Lawrence Rotch conceived the idea of establishing a meteorological observatory on the Great Blue Hill, near Boston. At first Mr. Rotch intended to use this observatory for special investigations, leaving the regular work to Signal Service observers. As no plan of cooperation with the Signal Service was found feasible, he determined to carry on the entire work under his own direction and at his own expense.

Mr. Rotch was particularly fortunate in his choice of a site for his observatory. Although the summit of the Great Blue Hill is but 635 feet above sea level, yet it possesses many of the characteristics of a mountain. It is the highest point of land in eastern Massachusetts, and offers an unobstructed view for many miles in all directions. This feature has been particularly valuable in prosecuting cloud studies.

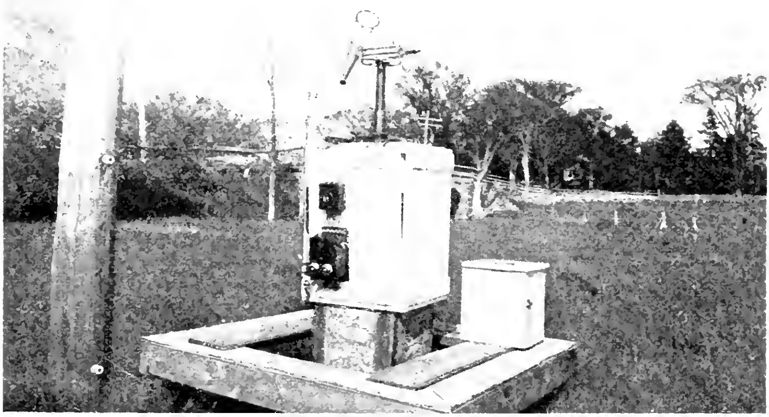
THE VALLEY STATION OF BLUE HILL OBSERVATORY.

The location is so near the coast that the characteristic water and land influences on the atmospheric conditions can be perceived. Moreover the summit of the hill is near that critical altitude at which the diurnal variation of the wind changes from the low level type to the high altitude type. We had meteorological records from the Signal Service stations on Mt. Washington (altitude about 6,000 feet) and on Pikes Peak (altitude about 14,000 feet), but we had none from the lower altitude at which the powerful local influence of the ground surface ceases to be overpoweringly effective. Thus this observatory fitted into a vacancy which it was desirable to fill. Nor is this all. The wonderful success attending the recent extension of the work of the observatory to the exploration of the upper air by means of kites has been in no small part due to the perfect adaptation of this locality for carrying on such

work. As regards location, the Blue Hill Observatory thus occupies a unique position among the meteorological observatories of the world.

Two secondary stations, at altitudes above the sea of 50 and 200 feet, respectively, at the base of Blue Hill bear the same relation to the main observatory that the base stations bear to the higher ones in the most completely planned European mountain observatory systems; while the Weather Bureau station at Boston and the neighboring Harvard Observatory meteorological station offer the advantages of representing the adjacent country.

Just as Dr. Wild set a pattern for Europeans to copy, so Mr. Rotch has given the United States a model observatory which it will be no mistake to use as a pattern in the future development of observational meteorology in this country. It seems to me that every possible precau-



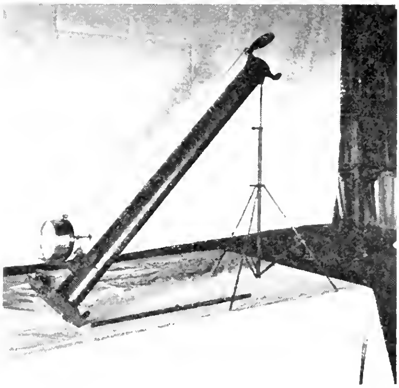
THE STATION FOR CLOUD MEASUREMENTS IN VALLEY, 1896-1897.

tion has been taken in the placing of the apparatus and in its convenient manipulation. The instruments and apparatus are of good construction and well adapted to the work required. The personnel of the staff could not be improved; certainly not in this country and probably not abroad. The true scientific spirit prevails at the observatory, and I have found there the same distinctive atmosphere which marks the Russian observatory at Pawlowsk. There can be no doubt but that the Blue Hill Observatory is the most successfully conducted meteorological observatory in America, and its work will compare favorably with that of European observatories of the highest class.

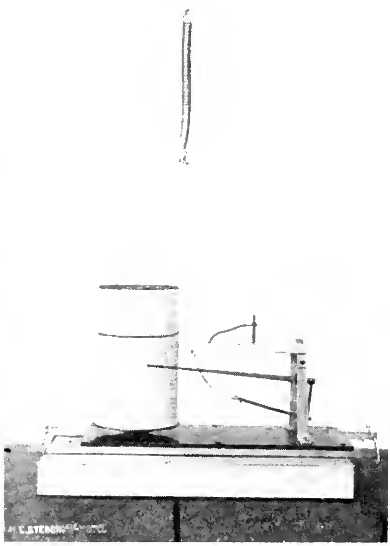
Some may wonder how it was possible for this observatory to have such a good start, reach such a high state of development within a brief space of time, and avoid those errors of organization and management

into which much more ambitious institutions had fallen. The reason is very plain to those who are familiar with Mr. Rotch's numerous visits to the best of the European central and mountain observatories. The care that he has taken to inform himself thoroughly in regard to their equipment, work and general effectiveness is clearly reflected in his printed descriptions of these institutions. By this means the youthful director of this new American observatory was enabled to take what might be termed a 'short cut' to leadership in our observational meteorology.

The regular work of the Blue Hill Observatory is carried on by Mr. Rotch with the assistance of Mr. H. H. Clayton, meteorologist, Mr.



THE POLE STAR RECORDER FOR REGISTERED CLOUDINESS AT NIGHT.

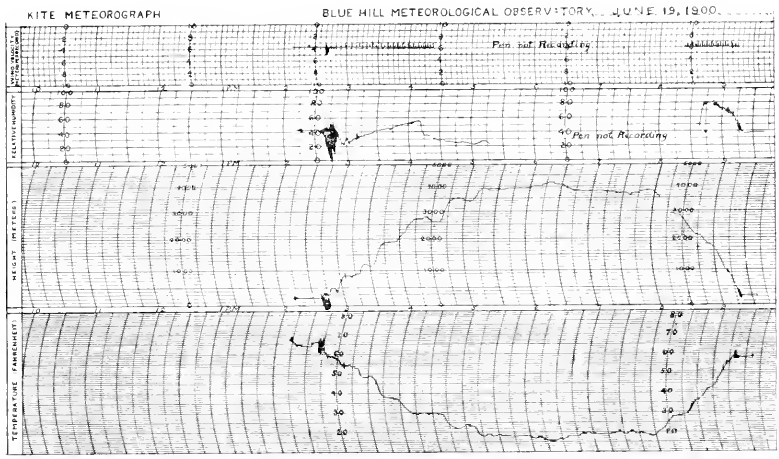


FIRST THERMOGRAPH LIFTED BY A KITE EMPLOYED IN 1891

S. P. Fergusson, mechanician, and Mr. A. E. Sweetland, observer. Not only did Mr. Rotch show excellent judgment in selecting a locality for his observatory, but he has shown equally good judgment in the choice of problems for investigation; he has taken up just those questions concerning which we have been sadly in need of numerical data, and to which every contribution is of distinct value. Moreover Mr. Rotch was exceedingly fortunate in his selection of capable co-workers, for they have responded in a notable manner to the demands which their science has made on them.

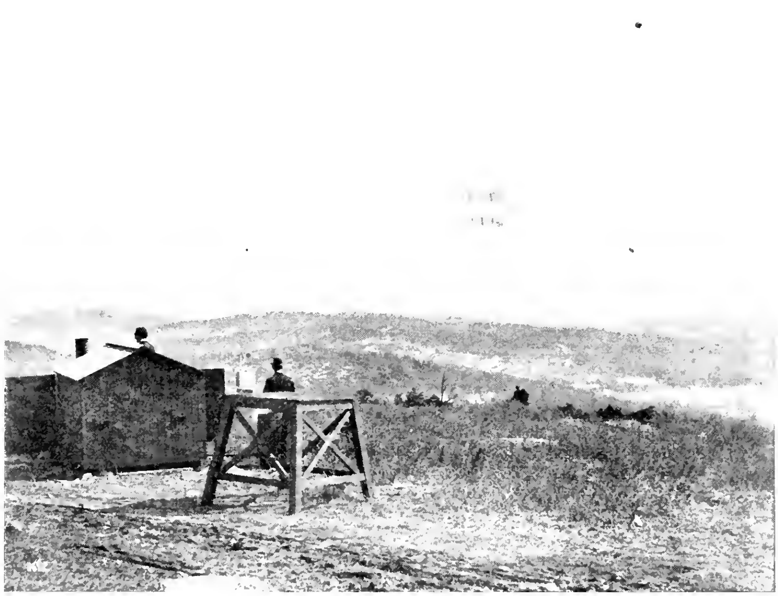
The investigations undertaken at the observatory may be divided into three classes: (1) The routine work of making observations of the local atmospheric conditions both by automatic registration and direct observation; and the reduction and publication of results. (2) The exploration of upper air by means of kites, and (3) Special studies of important topics by the observatory staff and visiting scientists.

The Blue Hill Observatory was established in 1885. Its report for 1886 shows the institution still in its formation period. The annual report for 1887, when it began to appear regularly in the 'Annals of the Harvard College Observatory,' is almost complete, lacking only the hourly values of the relative humidity of the atmosphere, but more than making up for this by the very complete hourly record of cloud observations (from 7 a. m. to 11 p. m.). In the report for 1888 all

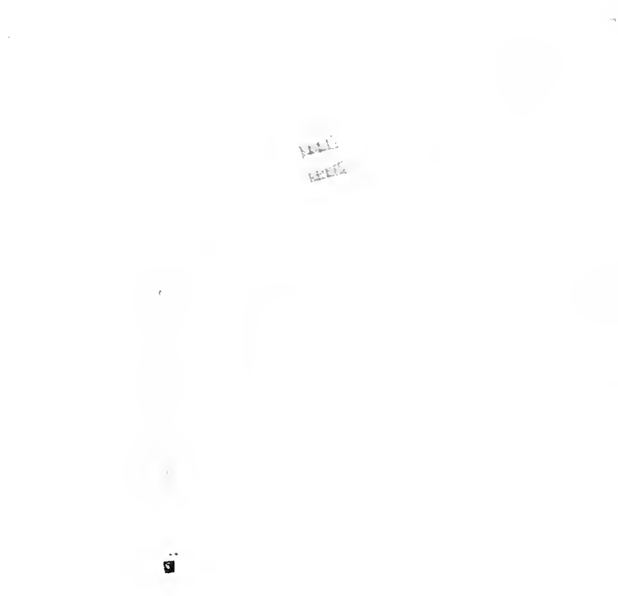


RECORD OF A KITE METEOROGRAPH.

the usual meteorological elements are given; but thereafter hourly values of the precipitation and cloud observations alone are given, the other elements being recorded only in summary. The reason for this long backward step, for it certainly is such, was probably the desire to economize the time required for reducing the observations and the cost of printing, although it may have been thought that the publication of the hourly observations *in extenso* during two or three years was sufficient. This latter is not the case, however, for what we most lack in the study of the air conditions in this country is reliable hourly observations conveniently accessible through print. It is especially desirable to have these records when they are as carefully made as those at the Blue Hill Observatory. It must be remarked, however, that the proper reduction of automatic records is a very laborious task.



Starting a Kite Flight on Blue Hill.



Kite Meteorograph in Air

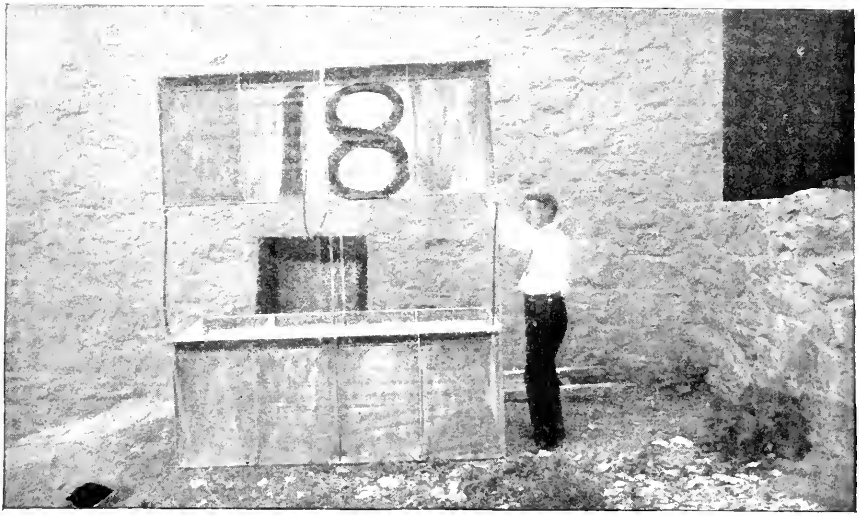
The successive years of continuous or hourly observations have permitted the determination of the diurnal and annual periods of the chief meteorological elements at the Blue Hill Observatory, summaries of which for several years' averages have been published. The main interest in these results centers in the air movements. The constancy of the local amount of wind from hour to hour has been found to be remarkable, and this, in connection with the variability in the hours of maximum and minimum wind, indicates the nearness to the transition altitude where the lower air conditions change to those of the upper air. These observations of wind velocity, coming in as they do at an intervening altitude between those of the ordinary high exposed surface station and the more elevated mountain stations, permitted the discovery of the gradual shifting towards noon of the hour of minimum diurnal wind velocity, with the gradual increase in altitude. Thus the least wind occurs at Boston at 5 a. m., at Blue Hill at 8 a. m., on the Eiffel Tower at 10 a. m. and shortly after noon on Mt. Washington and other similar high altitudes.

Much of the well-earned reputation of the Blue Hill Observatory depends on the special investigations which have been conducted by its scientific staff. Some of these are the natural concomitants of the peculiar location of the observatory, while others have been taken up on account of their intrinsic importance to meteorological science; still others combine these two features.

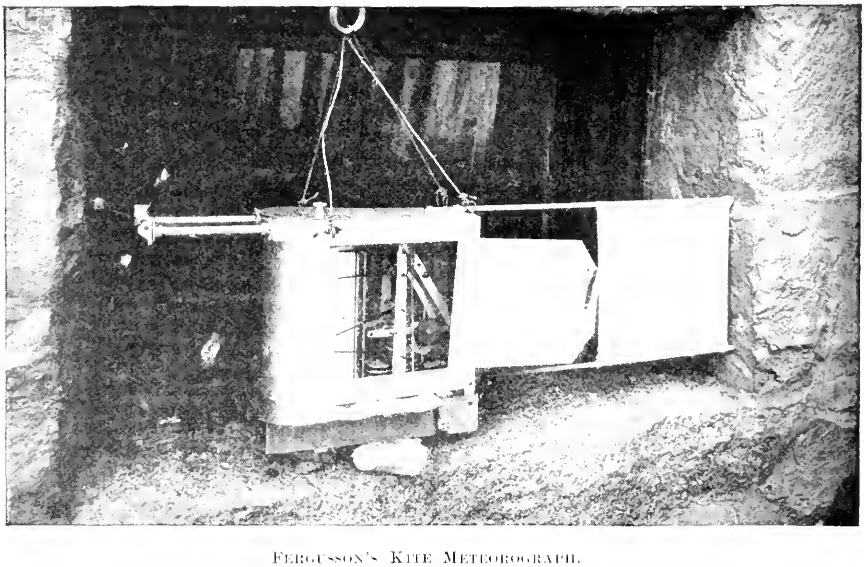
Among the questions taken up for the former reason, the following deserve special mention: The investigation of the normal differences of temperature between the base and the summit of the hill, and between the latter and the neighboring Weather Bureau station in Boston; the investigation of the marked inversions of temperature between the base and the summit stations; experiments on the electrical condition of the atmosphere; and studies of the vertical component of the wind as measured at the observatory.

The Blue Hill series of observations of visibility of more or less distant hills and mountains is very important, although the positive deductions as yet made from the data assembled in regard to this phenomenon are very meager. In general, however, it was found that the summer haze about balanced the winter fogs, so that an annual periodicity is but slightly marked. The diurnal period is also not clearly pronounced.

The location of the Blue Hill Observatory also made it a very desirable place at which to undertake open air experiments on the absolute and relative accuracy of anemometers. These were very much needed in view of the fact that the old errors of observed wind velocities could no longer be neglected when the comparatively recent quantitative study of the winds was widely taken up: and since much of the investiga-



Modified Hargrave Kite.



Fergusson's Kite Meteorograph.

tion designed to remedy this defect has been performed under artificial indoor conditions. These Blue Hill investigations showed plainly the necessity for greater uniformity in anemometers both as regards shape and size. The fan or bladed anemometers, with the use of ball bearings, seem to have many advantages over the ordinary cup anemometers now so generally used. There was found to be still much room for improvement in the pressure wind gauges, as those at present in use are not thoroughly satisfactory. The pressure tube anemometers, upon which many hopes have been built, showed need of some further modifications before it will be perfectly adapted to all conditions of wind and weather. Concerning the standardizing and testing of anemometers under artificial conditions, the opinion is advanced that a current of air produced by a blower is more likely to give absolute results than the whirling machine at present in use.

Among the important general meteorological questions taken up are the following: (1) The investigation of the temperature indications of thermometers placed in different kinds of thermometer shelters or screens. (2) The study of special phenomena exhibited by the records of self-registering meteorological instruments; such, for instance, as the dynamic effect of the wind on barograph records. (3) The study of weather predictions, from both the central and local points of view, and the demonstration that the combination of the two methods gave the best results. (4) The study of sudden falls of temperature and their relation to general atmospheric conditions. (5) The study of wave-like oscillations shown in the records of barometric pressures. (6) Studies concerning the periodicity of the weather. (7) The discussion of cloud observations, especially those made at the Blue Hill Observatory. (8) The improvement of meteorological apparatus, especially in the self-registering devices, and adapting the existing instruments to special needs. (9) The study of special cloud forms. (10) Cooperative study of clouds during the International 'Cloud year.'

That the movements of the atmosphere follow on certain laws we all recognize. Some of these laws we know, others remain still to be discovered. No work of the Blue Hill Observatory has exceeded in importance its studies of the actually observed movements of the air, and the so-called dynamic changes which these movements cause the air to undergo. In nearly every phase of this many-sided question this observatory has increased our stock of knowledge.

The study of the upper atmosphere was early begun by the staff of the Blue Hill Observatory. At first it was mainly carried on by means of cloud observations, but since 1894 by means of registering meteorological instruments carried aloft by kites. To this observatory belongs the honor of thus sending up into the air the first continuously

recording meteorological instruments used in this manner. Mr. Eddy, of New Jersey, used his kites in making this first trial. The work has been pushed with such success that records of atmospheric pressure, temperature, relative humidity and wind velocity have been secured by means of kites up to a height of 15,800 feet above the sea.

In a pioneer work of this kind, it was found necessary not only to modify old apparatus and methods so as to fit the novel applications, but also to devise new ones as well; and many of the details of the system as established at Blue Hill have been copied by meteorologists in the prosecution of similar researches both in this country and in Europe.

In this connection the Blue Hill studies of the clouds have led to the consideration of many problems to which these phenomena either directly or indirectly furnish a key. As in other studies carried on there, this work has been undertaken in the light of what has been done by other investigators; and in Mr. Clayton's report on the subject an excellent summary of what has already been accomplished introduces us to the more distinctively Blue Hill work. It has too frequently hap-

Evolution of the Kite Reel.

pened that the cloud work which has been done in different parts of the world has had its value much decreased owing to uncertainties in cloud nomenclature, and much of the recognized value of the Blue Hill work is to be attributed to the great care exercised in this preliminary matter. A valuable contribution has been made to the revision of cloud nomenclature, taking into account the elevations of the clouds. The annual and diurnal periodicity of clouds has been carefully studied on the basis of cloudiness at different levels.

In the study of the relation of clouds to rainfall are taken up: clouds preceding rain, clouds between intervals of rain and clouds following rain. The methods and cause of cloud formation were also carefully considered. The most important part of this special investigation is the use to which the cloud observations are put in the study of atmospheric dynamics, taking up in succession the questions: the relation of clouds to cyclones and anti-cyclones, having regard to the altitudes of the cloud levels; the annual and diurnal periods in the winds in general, at various levels as shown by direct observation near the ground and extended upwards to high altitudes by means of the observed cloud movements; the wind movements in cyclones and anti-cyclones from the ground up to the altitude of the highest clouds; the relation of the direction of the cirrus clouds to the existing temperature gradient; the relation between the velocity of storms, and the consequent variability of the weather, to the general movement of the atmosphere as shown by surface wind and cloud observations; the use of cloud observations in weather forecasts; and the frequency of winds from various directions at different heights above the ground, for different hours of the day, shown by wind and cloud observations.

The work of making observations of the atmospheric conditions in the free air by means of kites has been carried out with the success achieved only by the persistent endeavors of the observatory staff, not only in overcoming the difficulties in the mechanical construction of the apparatus employed, but also in the actual work of kite flying.

Experiments were undertaken as to the best forms of kites to use, the best materials for their construction, and the best lines to use for flying them. Special forms of self-recording meteorological instruments had to be so designed or so changed as to be adapted to the demands of kite work. Great care was exercised in so exposing the instruments that their possible errors would be reduced to a minimum. During the year 1897 there were thirty-eight successful kite flights, in 1898 thirty-five, in 1899 twenty-five, and in 1900 twenty-four; the average height above sea level at which records were obtained during the respective years being 7,350 feet, 7,400 feet and 8,450 feet, thus showing constant improvement in the methods employed.

The discussion of the Blue Hill observations has added very materially to our still meager knowledge of the distribution of the meteorological elements in the free air, and their variation with change in altitude. The average increase of wind velocities with increasing altitude was determined chiefly for those altitudes for which we have the fewest data because it is so difficult to make measurements there by means of the clouds. The change in direction of air currents at different levels was also clearly and accurately brought out by the changes in position of the kites as they ascended and descended. Such data as these are

particularly valuable for determining the effects of the friction of the ground on the winds, and for testing quantitatively the theories of the air circulation which have heretofore depended mainly on qualitative generalities.

The decrease (and in the abnormal cases, the increase) in temperature with height above the ground has been carefully studied, and especially in the various phases which occur under different typical atmospheric conditions. The diurnal changes of temperature at different altitudes have been also carefully studied. The determination of the numerical values of these elements is very important in helping to complete the theories of the atmospheric circulation, solar insolation, and the transference of heat from the earth to the air.

The rate of change of relative humidity with change of altitude, due to vertical change of temperature, is very important in connection with the calculation of the heights of clouds by computing the altitude of the dew point temperature under known conditions near the ground; and the Blue Hill observations not only offer data for increasing the accuracy of these calculations, but also a criterion for testing their absolute accuracy.

Until 1886 the only weather map in the United States was printed at the Chief Signal Office in Washington, but in May of that year Mr. Rotch with the assistance of Mr. Cole, the government observer in Boston, began to chart the 7 a. m. reports that were received there, and manifolded the map by the cyclostyle process. This was the origin of the daily weather map that is now issued in great numbers from many of the Weather Bureau Stations throughout the United States.

From 1887 until 1891 local weather forecasts were furnished by the Blue Hill Observatory to the Boston press and announced from the observatory by the display of weather signals. These weather predictions were undoubtedly a considerable improvement over those made in Washington, which depended on the weather map alone, especially for the twenty-four hours immediately succeeding the time of observation, and the demonstration of this, in direct competition with the Weather Bureau predictions, probably had some effect in causing the government service to appoint local forecast officials to supplement the general predictions made at Washington. It must be borne in mind in this connection, that this combined method of making weather predictions has been, in a measure, practically carried out in European countries ever since an international telegraphic exchange of weather observations went into effect. In this country, however, we had learned to rely too much on the general predictions issued from Washington.

There can be no doubt that the work of the Blue Hill Observatory has had a very great quickening influence in the recent developments in observational meteorology in this country. Not only has its thor-

oughly independent attitude and scientific spirit enabled it to make usefulness and not policy its watchword, but it has also permitted it to improve the older traditions of American meteorology, by adding to them the best features of European meteorology.

So far as concerns the regular routine work of observation of the purely local atmospheric conditions made at the Blue Hill Observatory, it is impossible to realize its importance to American meteorology under the light of present conditions alone. One must go back twenty years to the conditions existing in the early eighties to properly appreciate its innovating character. Concerning the extra routine work, such as the studies of the upper air conditions and their application to atmospheric mechanics, no comment seems necessary further than to mention the fact that this work occupies a prominent position in the front line of scientific advance in this direction. We have had a recent example of this in the discoveries attending Mr. Clayton's studies of eclipse meteorology, in which important extensions of Ferrels' cold centered cyclone have in all likelihood been made which will greatly aid in the solution of some hitherto unexplained meteorological problems.

The importance attached by scientists to the work of the Blue Hill Observatory is plainly indicated by the numerous long and appreciative reviews and notices of this work which have appeared in such general scientific journals as 'Nature' and 'Science,' and such special journals as the 'Meteorologische Zeitschrift' and the 'American Meteorological Journal.' Probably not one of the long list of reports and other publications of the observatory has been passed by without printed comment, and frequently a single paper has called forth several reviews. Moreover the confidence with which this work has been received is shown by the fact that the results have been freely used by subsequent investigators.

It is a noteworthy fact that by taking the initiative in the systematic sounding or exploration of the upper air by means of kites, the Blue Hill Observatory has added one more feature to the already long list of American pioneer contributions to meteorology, among which may be mentioned, Loomis' storm and weather maps, Espy's dynamical theories, Ferrels' theories of the atmospheric circulation, and the more complete extension of the application of weather knowledge to practical affairs by the Signal Service and Weather Bureau.

The official publications of the observatory are: the earlier annual reports published separately and the later ones published since 1887 in the 'Annals of the Observatory of Harvard College,' special memoirs and discussions forming an important feature of these reports; monthly and annual summaries of the climatic observations, which were manifolded and distributed locally up to 1896, and some of which the 'U. S. Monthly Weather Review' and the monthly report of the New

England section still publish; occasional Bulletins containing original memoirs that it was desired to publish promptly on special meteorological topics.

In addition to the preparation of these official reports, the members of the observatory staff have published a great number of letters, articles, and reports in the journals of general and technical science both in Europe and America. This individually published material has always been of that high character which bears internal evidence of the earnestness and ability of the authors, and it has always received from scientists both at home and abroad the careful consideration due it.

The high character of the work undertaken and the great amount accomplished by steady application during the fifteen years of its continuance have given the Blue Hill Observatory a position among the best observatories of the world. There are certainly very few even of the great national meteorological observatories which are better known or are held in higher esteem than the private observatory established and maintained by Mr. Rotch on the highest summit of the Blue Hills of Milton.

In closing this article I venture to express the opinion that when the history of meteorology during the latter part of the nineteenth century is written, the Blue Hill Observatory will be assigned the foremost place in American observational meteorology, and this judgment will be based not only on the observations which have been made, but also on their proper discussion and correlation with allied branches of this science of the atmosphere.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

A NATIONAL association for the advancement of science occupies at the beginning of the twentieth century a dominant position. The greatest achievement of the nineteenth century was the progress of science; its most definite tendency was towards the voluntary organization of individuals for the accomplishment of certain ends. The advance of science, the movement that is of the greatest importance for civilization, requires for its guidance the strongest association of individuals. Such an association will certainly arise, and will develop from existing institutions.

The organization of science in America has progressed parallel to the advance of science. Local societies concerned with the whole field of knowledge, and especially with its utilitarian aspects, were first established in Philadelphia, in Boston and in other cities. These societies were modeled on the similar institutions of Europe; the Philosophical Society of Philadelphia following the Royal Society of London, and the Academy of Arts and Sciences of Boston, the Paris Academy of Sciences. As centers of scientific activity increased in number, as the postoffice and railways developed, as general scientific journals were established—'The American Journal of Science' began publication in 1818—the need of a national organization was felt, and here again the older nations had established the precedent. The meetings of German scientific men and physicians began in 1828, and the British Association for the Advancement of Science was established in 1831. An Association of American Geologists and Naturalists was organized in 1840, and became the American Association for the Advancement of Science in 1848.

Fifty years ago the sciences were comparatively undifferentiated. Special societies and special journals were not required. It was pos-

* We reproduce this article from advance sheets of 'Science,' as all our readers will be glad to have brought to their attention the question of the organization of science in America. The American Association for the Advancement of Science meets this year in Denver, further to the west than ever before, and its influence and membership should be greatly increased in the states west of the Mississippi River. Nothing is more gratifying to the conductors of this Journal than its large circulation in the middle and western states, and we hope that many of those who read this article will become members of the American Association. The conditions of membership can be obtained from the permanent secretary, Dr. L. O. Howard, Cosmos Club, Washington, D. C.—EDITOR.

sible for students of science and friends of science to meet together and take a common and intelligent interest in the scientific progress of the day. Somewhat later, however, the need became apparent for a more select national society. The local academies in the European capitals had become national institutions in a way that was not possible for the similar societies in the United States, owing to the lack of centralization. Our National Academy of Sciences was organized in 1863 with a membership at first limited to fifty, and still under one hundred. The Academy was intended to be the adviser of the Government in scientific matters, and has to a certain extent fulfilled this function. At first, when there were but few scientific men in the United States and their interests were more or less common, the National Academy was an organization fitted to its environment. But it has scarcely adjusted itself to the growth and specialization in science of the past twenty-five years.

The organization of science that was adequate for the third quarter of the century did not suffice for the fourth quarter. About twenty-five years ago it became necessary to meet the specialization becoming inevitable for scientific advance. Special societies and special journals were organized. The American Society of Naturalists, organized in 1883, and the 'American Naturalist,' established in 1867, covered a limited, but still wide field. 'Science,' a weekly journal, was established in 1883 to keep the sciences in touch with each other and men of science in touch with the general public. The American Chemical Society was organized in 1876, The American Ornithologists' Union in 1883, The Geological Society of America and the present American Mathematical Society in 1888, and there are now national societies for almost every science. Special journals were established during the same period—'The Bulletin of the Torrey Botanical Club' (1870), 'The Botanical Gazette' (1876), 'The American Journal of Mathematics' (1878), 'The American Chemical Journal' (1887), 'The American Journal of Morphology' (1887), 'The American Journal of Psychology' (1887), 'The American Geologist' (1888), 'The National Geographic Magazine' (1888), 'The American Anthropologist' (1888) and so on, in increasing numbers, to the present time. A similar movement toward specialization is evident in the development of elective courses in our colleges, of advanced work in our universities, and in many other directions.

The American Association for the Advancement of Science did not fail to adjust its organization to the growth and differentiation of science. In 1875 a formal division was made into two sections, one for the exact and one for the natural sciences, and in 1882 nine sections were established. At this time, when the Association had fitted itself to existing conditions, it enjoyed a most prosperous period in its his-

tory, the meetings being large and fruitful. Thus the attendance at Boston in 1880 was 997; at Montreal in 1882 it was 937, and at Philadelphia in 1884 it was 1,261. But with the organization and growth of the special societies and journals referred to above, the Association did not maintain its commanding position. The American Society of Naturalists, with a more compact membership, chose midwinter as its time of meeting, and other societies became affiliated with it. The special societies, consisting of groups of experts, appealed to the loyalty of their members more directly than did the larger and more amorphous Association. There was even lack of sympathy between these societies and the Association. The attendance at the meetings became smaller, and the total membership decreased. The more eminent men of science and the younger workers were not regularly in attendance at the meetings and were perhaps not even members of the Association. The programs of the sections became heterogeneous and sometimes did not reach a very high standard. The amateur and picnic elements were rather prominent, while at the same time they were mediocre. Many men of science regarded the Association as a survival that had outlived its usefulness.

But to-day no one acquainted with the most recent work of the Association will deny that it has entered on a new period of its history. This began with a change of attitude toward the special societies, replacing rivalry with cooperation. There was much opposition to the plan of letting the American Chemical Society meet in affiliation with the Association, but when this was accomplished chemistry at once became its strongest section. So it has been in other cases, where special societies have met in affiliation with the Association. At the recent New York meeting there were sixteen such societies including practically all national societies that hold summer meetings. Other improvements in the organization of the Association have been effected. The council has been strengthened and made a truly legislative and executive body. The permanent funds have been increased, and appropriations for research have been granted to committees. Care has been exercised in the election of fellows, and in the admission of titles to the programs. 'Science' is sent free of charge to all members, thus increasing and consolidating interest in the Association and in the advancement of science, giving even those unable to attend the annual meetings an adequate return for membership, and tending to unite all men of science and those interested in science in the Association and in the ends that it represents. The last three meetings, held at Boston, Columbus and New York, were all excellent, representing different types adjusted to the occasion and place. The meeting at Denver this year will be equally typical and equally successful. The membership of the Association is now larger

than it ever was before, over eight hundred new members having been elected within the past year.

There is every reason for satisfaction at the present condition and outlook of the Association. But this does not mean that we need not be on the alert to increase its usefulness under the circumstances confronting us at the beginning of the twentieth century. Evolution occurs by natural selection, but with boundless waste, regardless of time and careless of the individual. Human development must henceforth be guided by forethought and reason. It is the object of this article to make some definite suggestions regarding the organization of science in America under the auspices of the Association. They have been carefully considered by some of those most interested in the Association and, though they may not meet with universal approval, they are thought to be worth careful consideration.

The objects of the Association are said in its constitution to be "by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men, increased facilities and a wider usefulness." This statement may be somewhat systematized and amplified. The legitimate objects of the Association may be said to be (1) the presentation and discussion of research work in the different sciences and the publication of such research. (2) The promotion of research by grants of money and by providing the means for cooperation. (3) The encouragement of addresses, reports and publications on the progress of different departments of science, sometimes of value to the specialist, but more especially important in keeping the sciences in touch with each other. Joint meetings, discussions and publications should be arranged on subjects common to different sciences, relating the pure and applied sciences or concerned with science as a whole. (4) The presentation of such addresses, reports, discussions and publications in a form that will so far as possible keep the general public informed on the advances of science, interest them in the opportunities of scientific work and its needs, and impress on them the dignity and supreme importance of science. Here should be included whatever will secure recruits to scientific workers and the money and support that scientific work requires. (5) Offering an opportunity for men of science in different departments to become acquainted personally and by publication, and encouraging their sympathy and loyalty to their common interests and performing, so far as possible, the same function for scientific men and the intelligent public. (6) The guidance of scientific organization in America, which includes the coordination, establishment and arrangements for the meetings, etc., of special scientific societies; the

publication and circulation of scientific books and journals; the place of science in education and all external means for the advancement and diffusion of science; the direction of public opinion and legislation on science, more especially when connected with the national government, and the different states and municipalities; the promotion of conditions required by science and of reforms recommended by science —in general, whatever will promote the advancement, diffusion and usefulness of science.

1. The first of these functions has in large measure been assumed by the special societies and journals, and this is in accordance with necessary conditions. Special research must be presented before, and discussed by, small groups of experts and must be published in journals that are of interest only to specialists. The special societies have compact organizations; they are most competent to select their membership, to arrange their programs and to conduct their publications. It seems inevitable that the Association must relinquish its function of providing sections for the presentation of special papers, except in the rare case that a special society does not exist and may be formed by the aid of the Association. In a joint meeting of a special society and the corresponding section all the valuable papers will be presented both before the society and the section, and only such papers will be presented to the section alone as the society will not admit. There is, however, no reason why the present general organization should not be maintained, and the papers read before the affiliated societies be made part of the proceedings of the Association. The Association may, however, render important assistance to the special societies in the ways indicated below.

2. The promotion of research by grants of money and by providing the means for cooperation is a function that should be undertaken both by the special societies and by the general Association. The latter is, as a matter of fact, more likely to secure funds for this purpose by bequests and gifts, owing to its national character, its long history and its permanence. It can to special advantage further researches in which more than one science is concerned and in which independent societies might fail to cooperate. Efforts should be made to increase the number of patrons of the Association and to secure bequests and gifts, in order that the American Association may not be behind the British and French Associations, which appropriate annually $5,000 or more for the direct encouragement of research. Invested funds yielding an income for this purpose would add greatly to the stability, influence and usefulness of the Association, and to the interest of the meetings at which the grants are made and the reports of the work accomplished are presented.

3. The special societies may with advantage present addresses and

reports on the progress of a science, and, when the societies meet at the same time and place, their value is increased by the opportunity afforded for a larger group to be present. In this direction the Association has, however, an important work. The address of the president, the most eminent man of science in America who has not yet held this office, should be an event of national importance. It should be worth publication, and should be published in full in all the important daily newspapers, as actually happens in England in the case of the president of the British Association. The addresses of the vice-presidents should be as nearly as may be of the same importance and interest. These should not be addresses such as are presented before the special societies, but should be intelligible and interesting to all men of science and to the great mass of men and women who have had a college education or an equivalent training in affairs. The afternoons through the week might with advantage begin with these addresses, not more than two being given simultaneously, and these might be followed with reports or discussions of problems of general interest. The sectional committees and the council should pay special attention well in advance to the arrangement of a program. Care should be taken, if necessary, by invitation to those not members of the Association, to secure the adequate presentation of subjects in which the Association needs strengthening. Thus applied science should be given more prominence than hitherto. Those eminent in public life, in educational work and the like, and distinguished foreign men of science might be invited to address the Association or to take part in its discussions. Funds should be available to defray at least the traveling expenses of such invited guests.

4. The addresses, reports and discussions should, in part at least, be of such interest as to attract the general public, securing a large local attendance and being reported widely by the press. It is not possible, least of all in a democratic country, for science to isolate itself from common life. There must be special research that can be appreciated only by the expert, but as quickly as possible the progress of science should be made a part of the world's common stock of knowledge. The American Association should be one of the chief factors in the diffusion of science, and its annual meetings should be looked forward to by the general public as the occasion when for its benefit the year's progress in science and the contemporary state of science are exhibited in their outlines and in correct perspective. The meetings should typify the dignity and weight of science, so as to impress these on the minds of all. The sympathy and support of all the people are absolutely essential for science. Only so can recruits for scientific work be secured; only so can endowments and material support be obtained; only so can scientific work under the government be placed on a secure and permanent basis. We have in these needs not only the

justification, but the absolute necessity of an Association with a large membership—it should be at least ten thousand—drawn from the intelligent people of the whole country.

5. The social intercourse and personal contact of scientific societies and meetings are among their most important functions. Men in isolation become selfish and incompetent. Even a great genius does not work in solitude, and certainly the ordinary man requires the interest and enthusiasm that is only evoked in the give and take of personal acquaintance and conversation. Eating together, drinking together, smoking together, may have physiological drawbacks, but the psychological stimulus has warranted the origin and survival of the practices. Those studying similar problems, and those working in diverse directions; the university professor, the school teacher and the government officer; those who call their science pure and those who seek to make it useful; the beginners and the old benchers, all should be thrown together, ready to learn and help, to agree and differ. Each should be prepared to profit much, and if need be to sacrifice a little for the common good. The meetings of the Association do, of necessity, accomplish a great deal in bringing men together, but perhaps not all that could be desired. The cultivation of personal acquaintance between professional men of science and the amateur and outsider is also important, but more difficult to manage. The social features of the British Association seem to be more successful than our own. A thousand or more of the leading citizens of the place become temporary members for each meeting, and freely offer entertainments of one sort or another. The social conditions are, of course, different in America, but it seems that the entertainments and excursions might be made more pleasant and profitable in the future.

6. Of all the important functions of a national scientific association, the most essential is the general organization of science. The science of the country absolutely requires a central legislative body. Such bodies exist in other nations, having varying degrees of usefulness, and there is more need of an active and efficient representation of scientific interests in the United States than in any other country. London, Paris and the other European capitals, with their societies, clubs, etc., bring together all the scientific men of the country, whereas here they are widely scattered, and will become still more so as the East loses its intellectual precedence. Washington will doubtless be our chief center for scientific research, but under our system of State governments and with our privately endowed institutions, it is not likely that it will occupy the position of European capitals. The great development of scientific work under the national government, the numerous smaller centers under the State governments at their capitals and universities, the municipalities with their increasing

tendency to support museums, libraries, etc., and to undertake functions requiring scientific experts, the great incorporated universities developing special research, the applications of science in industries, transportation, etc.—all these represent an extraordinary activity, and, at the same time, a dispersion of tendencies and interests that require here more than in any country some unifying and centralizing organization. The functions of such a body are only limited by its efficiency. Our government recognizes a division into executive, legislative and judicial functions, but does not recognize the coordinate importance of expert opinion. As the judicatory interprets the laws made by the legislature, so the legislature requires impartial advice and scientific knowledge as the basis of its enactments.

The question now arises as to what body or bodies should perform the functions thus outlined. In the first place, it is evident that we need numerous and partly independent institutions. Each university, museum, survey, observatory, botanical garden, laboratory and the like is a unit, requiring its special organization. Each city should have a local academy, or alliance of societies, which in its field should perform most of the functions that we have been considering. Similar academies, or groups of societies, are needed for a State or region. National societies are required for each science. But what should be the national organization that will bring all the local and special societies together, and accomplish for the nation and for science as a whole what these institutions and societies do for a locality or a single science? We have at present the National Academy of Sciences and the American Association for the Advancement of Science, both of which have to a certain extent filled these requirements, but only in a partial and imperfect way. The Academy is legally the adviser of the government, the Association has brought into its organization a majority of the scientific men and many of the scientific societies of the country, but it seems probable that neither a small self-perpetuating body of eminent men nor a plebiscite of all scientific men will perform the duties required. Representative government, in spite of its partial failures, is the kind of government under which we should live and must live. We find this most nearly embodied in the council of the American Association. This council might be made the representative body for science in America.

If it be asked what the American Association and its council should do to assume the position assigned to them, the reply may fortunately be made: let them continue the work that they have already begun. The whole matter is one of attitude and spirit, rather than of constitution and by-laws. Let all scientific men be fellows of the Association, make the members representative of the intelligence of the country, unite all scientific societies and institutions in the organiza-

tion of the Association, make the meetings important and interesting, let the council assume and deserve authority.

While the position of the Association must depend chiefly on natural fitness and development and on the spirit and character of its members, there are certain changes in organization that deserve consideration. We shall suggest some modifications which appear to be either desirable at present or objects to be kept in view.

Affiliated societies should be represented on the council, and all scientific societies, whether national or local, should be affiliated with the Association. The number of representatives allowed from each society should be proportional to the number of members of the society among the fellows of the Association. For example, each institution having ten fellows might be allowed a representative and an additional representative for each additional twenty-five fellows. This plan includes the representation of local academies, universities, government departments, etc., on the council, but might begin with the societies meeting with the Association, in accordance with an amendment to the constitution now pending. It might be well for the council to elect each year three additional members to serve for a term of three years. Those so elected would probably be among the most efficient members of the council. The council would thus be considerably enlarged, but its authority would be greatly increased. It is of course understood that the real work of legislative bodies is done by committees, and the committees of the council should be organized with special care.

The executive officer of the Association is the permanent secretary, and his influence should be very great. He should either be paid a reasonable salary, say $5,000, and devote his whole time to the Association and the organization of science in America, or should be, as our present secretary, a man of unusual executive ability, having under him one or two assistant secretaries who should devote themselves to the work. The secretaries of the sections should be among the most efficient members of the sections, and should be elected for a term of three years and re-eligible.

The meetings should be more thoroughly organized in advance, more authority being vested in the permanent secretary and council. As suggested above, public lectures and discussions on the important advances and current problems of general interest should be arranged. For example, this year there should be reports on the relation of mosquitoes to disease, on the newly established Bureau of Standards, on the conduct of a national observatory, on the natural history and resources of the West Indies and the Philippines, and, in view of the place of meeting, on mining and irrigation.

The time of meeting has always interfered with success. Men of

science will not and cannot come together at midsummer. If a week can be set aside at the beginning of the year, it is probable that the scientific character and weight of the meetings will be greatly forwarded. The importance of obtaining a convocation week in midwinter has been emphasized in a recent editorial ('Science,' April 26, 1901), and we are now able to report that, of the fourteen universities composing the Association of American Universities, all but two either already have no exercises at the time or have altered their calendars in the direction of setting aside the week in which New Year's Day falls for the meetings of scientific and learned societies. It might, however, be well to have, say once in three years, a summer meeting in which the social and excursion elements should be emphasized. It must be remembered that the National Educational Association can bring together 10,000 members in this way. Or perhaps, it will be found with experience that the winter meeting is so advantageous that the summer meetings can be omitted altogether. Meanwhile there might be suggested a special meeting at Chicago next year at Christmas time in conjunction with the Naturalists and affiliated societies, the usual meeting at Pittsburg in midsummer, and a meeting of unusual importance at Washington at the end of the year.

SCIENTIFIC LITERATURE.

A HISTORY OF THE THERMOMETER.

Dr. H. Carrington Bolton is one of the few Americans acquainted with the history of science, and his little volume on the evolution of the thermometer (The Chemical Publishing Company) represents a type of publication too rare in this country. The scientific information is correct throughout and is based on first hand knowledge, while at the same time the contents are sufficiently interesting to be read by any one. It is probably not generally known that the thermometer was invented by Galileo. When we remember that we owe to this one man not only the foundations of physical science, but also in large measure the pendulum, the compass, the telescope and the microscope, it may lead to a certain amount of modesty in our appreciation of modern inventions. Galileo, probably in 1595, invented the open air thermoscope; he determined the relative temperature at different places and at different seasons of the year and made experiments on freezing mixtures. In 1611 Sanctorius applied Galileo's instrument to the diagnosis of fevers. Ferdinand II. of Tuscany, to whom we owe the famous 'Accademia del Cimento,' first sealed the glass, making the instrument independent of atmospheric pressure. Many improvements were gradually made especially in the endeavor to find fixed points on a definite scale, the freezing point of water being first used by Robert Hooke in 1664. Of the three thermometers still in use, Fahrenheit's thermometer was invented in 1709, Réaumur's instrument in 1730 and the scale of Celsius in 1742. None of these thermometers, however, are now used in the form in which they were originally devised, and Dr. Bolton calls attention to the somewhat curious fact that the instrument constructed by the German, Fahrenheit, is used almost exclusively by English-speaking peoples; that invented by the Frenchman, Réaumur, is used chiefly in the north of Europe, while that of the Swede, Celsius, is used in the French-speaking countries. Dr. Bolton does not attempt to compare the usefulness of the three scales. The centigrade scale is, of course, the most logical, but, as sometimes happens in this world, it is not quite certain that it is the most convenient. When the scale of temperature between freezing and boiling water is divided into one hundred parts, the degrees seem to be somewhat too large for use in daily life, whereas if it were divided into one thousand parts they would be obviously too small. It is possible, however, that this is Anglo-Saxon prejudice, and that the centigrade degree measures temperature with sufficient accuracy for ordinary purposes, while its decimal subdivision must certainly be used hereafter for scientific work. Dr. Bolton's book is so small that it seems a pity that he did not add a chapter on the exact thermometric methods of the nineteenth century.

EXPERIMENTAL PSYCHOLOGY.

Professor E. B. Titchener, of Cornell University, has given us our first adequate laboratory manual of experimental psychology and has thus marked an epoch in the development of a science. Experiment in psychology, like much else, goes back to Aristotle, and has never since been entirely lacking. The great philosophers—Descartes, Hobbs, Kant and the rest—advanced

psychology as well as the other sciences. When the separate sciences developed, some part of psychology was taken with them, and the physicist, the physiologist and the zoologist made experiments and researches which are now claimed by psychology. In the meanwhile philosophy continued to care for psychology—we have, for example, in England Locke, Berkeley and Hume, and in Germany Herbart—but sometimes without sufficient attention to observation and experiment. Then about fifty years ago, in the hands of those who cared both for philosophy and science—Lotze, Fechner, Helmholtz, Wundt and others—psychology took definite shape as a natural and experimental science. Wundt's 'Physiologische Psychologie,' published in 1874, was the first comprehensive handbook. James's 'Principles of Psychology,' published in 1890, is equally important and more readable. Apart from numerous good text-books and treatises in various languages, we had the first laboratory manual in Sanford's 'Course in Experimental Psychology' (1894), but this only treated the senses which had already been pretty well worked over by physicists and physiologists. Now in Titchener's 'Experimental Psychology: a Manual of Laboratory Practice'—the work is published by the Macmillans—we have the first complete laboratory course in psychology. It is a large work: Volume I, which has alone been issued, includes two parts treating qualitative experiments, one intended for the student (xviii+214 pp.) and the other for the instructor (xxxiii+456 pp.). Two further volumes, treating quantitative experiments, are promised. The experiments are described in chapters entitled: Visual sensation, Auditory sensation, Cutaneous sensation, Gustatory sensation, Olfactory sensation, Organic sensation. The affective qualities, Attention and action, Visual space perception, Auditory perception, Tactual space perception, Ideational type and the association of ideas, Appendices.

Detailed comment and criticism must be relegated to the special journals. There is no question but that the work will greatly forward the teaching of experimental psychology and is invaluable to the teacher and advanced student. There will be difference of opinion as to how far the book can be put to advantage in the hands of students beginning laboratory work in psychology, and the question can only be settled by actual trial. There is naturally less agreement as to what experiments should be made and what methods should be used than in the case of sciences, such as chemistry and physics, where natural selection has long been at work. But Professor Titchener has laid the foundation on which future workers must build.

THE PROGRESS OF SCIENCE.

THE JOHNS HOPKINS UNIVERSITY AND PRESIDENT REMSEN.

The election of Professor Ira Remsen to the presidency of the Johns Hopkins University has been received with general approval, and will be particularly gratifying to those who have been connected with the University as students or teachers and to men of science throughout the country. The Johns Hopkins University was incorporated in 1867; the founder died in 1873, and a year later Dr. D. C. Gilman was elected to the presidency. When the University opened its first session in 1876, Dr. Gilman had secured the services of a small but notable group of professors, of whom, since the lamented death of Rowland, but two remain—Professor Remsen and Professor Gildersleeve. President Gilman and his associates, freed somewhat from traditions and from the need of conducting a school for boys, erected at Baltimore a true university, distinctly in advance of any other American institution, except possibly Harvard, then just becoming subject to the influence of President Eliot—like Remsen, a professor of chemistry. In spite of the loss of a great part of its endowment, due not to carelessness on the part of the trustees but to the dictates of the founder, the Johns Hopkins University has maintained its position, and in the establishment of its medical school in 1893 has accomplished for medical education what had been accomplished earlier for university work. In the development of the university, Professor Remsen has always been President Gilman's chief associate and adviser, and is his natural successor. Remsen was graduated from the College of the City of New York in 1865 and received his M.D. from the College of Physicians and Surgeons, Columbia University, two years later. Studying abroad, he was made assistant in chemistry in Tübingen and was afterwards professor in Williams College, till his removal to the Johns Hopkins University in 1876. He has been given the LL.D. by Columbia and Princeton; is the foreign secretary of the National Academy of Sciences, and a member of many scientific societies. In his chemical laboratory and in 'The American Chemical Journal,' Professor Remsen has always upheld and forwarded the best ideals of research. As president of the Johns Hopkins University, he represents the highest type of educational leadership.

ACADEMIC FREEDOM HERE AND ABROAD.

Questions of academic freedom and the relations of university professors to authority are fully as troublesome abroad as in this country. It might be supposed that our system would work badly. The faculties have very little power, the authority being lodged in an absentee board of trustees and a president with almost absolute power; the trustees being usually and the presidents often men of affairs rather than scholars. The state universities are subject to political control, and the private universities are generally denominational and always dependent on the charity of patrons. Yet thanks to common sense and an appreciation of the importance of individual freedom, the university professor has in America a reasonably satisfactory status. There is little or no interference with the conduct of his department; his advance-

ment depends chiefly on efficiency rather than on favor; his position is permanent; he has complete freedom of research and reasonable freedom of speech and conduct. Those who are dissatisfied with the conditions here should make themselves familiar with what is happening abroad. We have recently had occasion to call attention to the troubles in the Royal Engineering College at Coopers Hill. Half the faculty was dismissed without a hearing by a board of visitors and a president, an army officer without academic experience. At this institution it appears that the professors are not even consulted as to the curriculum. An eminent chemist was dismissed from the University of Paris, because he believed that Dreyfus was not justly convicted; an eminent zoologist was compelled to leave the University of Zurich, because he took part in temperance reforms; now in Germany, supposed to be the home of academic freedom, we have events that could scarcely happen in America. The chair of zoology at Erlangen being vacant in 1897, the Bavarian 'Landtag' expressed the wish that the representatives of the natural sciences in the Bavarian universities should not be evolutionists. The associate professor of zoology at Erlangen, Dr. Albert Fleischmann, had published in 1896, the first part of a text-book of zoology based, like all recent works, on the theory of evolution. But when the second part was published in 1898, there was a remarkable conversion; a special chapter on the theory of evolution was added, declaring the theory to be absurd. The author was promoted to the professorship of zoology, and has now published a book, entitled: 'The Theory of Evolution: Popular Lectures on the rise and fall of a scientific hypothesis, delivered before students of all the faculties.' The book is not addressed to scientific men, but to the laity and clergy to whom the author owes his chair at the University of Erlangen.

SCIENTIFIC AND EDUCATIONAL ENDOWMENTS.

Two gifts of great importance to science have been made during the past month. Mr. Andrew Carnegie has created a fund of $10,000,000 for the Scottish Universities, and Mr. J. D. Rockefeller has established in New York an Institute for Medical Research. Mr. Rockefeller, in the endowment of the University of Chicago, has enjoyed the honorable distinction of having made the largest gift for public purposes, but even his great benefaction has now been surpassed by Mr. Carnegie.

The fund for the Scottish Universities has been transferred to trustees, in whose wisdom there will be perfect confidence, and no unwise restrictions have been placed on their power. At present, however, the income will be divided between paying the fees of students at the Scottish Universities, and strengthening the equipment and teaching staff. The scientific and medical departments, and modern languages and history, are designated as the subjects on which the money is to be spent. The Scottish Universities, like our own institutions, have always been close to the people, a very large percentage enjoying the benefits of a college training. An annual income of $500,000, devoted to higher education, will mean much for a comparatively small population.

Mr. Rockefeller's gift of $200,000 for an Institute for Medical Research is comparatively small. It is, however, intended for current expenses, and an endowment will doubtless be provided when required. The institute will be situated in New York City, but a building will not be erected at present, research being conducted in existing laboratories. The board of directors, with Dr. W. H. Welch of the Johns Hopkins University as president, guarantees the conduct of the institute in accordance with the highest scientific standards. It is most fortunate that we should now be on the way to share

with European countries the duties of organized medical research. There are institutions, such as Mr. Rockefeller has now founded, in Paris, Berlin, St. Petersburg and elsewhere, and last year Lord Iveagh established a similar laboratory in London. It is somewhat remarkable that great sums should have been spent annually on research in astronomy, geology and in other directions, whereas the advancement of medical science and its applications should have been left chiefly to individual effort. The first duty of the practising physician is to his patient, and the teacher in a medical school is doubly burdened, as he usually practises medicine, and at the same time instructs large classes. It is not surprising that the four hundred medical journals published in the United States are mostly somewhat dreary and barren, but, rather, that so much has been accomplished without state or private endowment.

THE CAUSES OF YELLOW FEVER AND OF CANCER.

What can be accomplished by properly directed medical research is proved by two advances of extraordinary importance made recently by American students. These are the discovery of the probable causes of yellow fever and of cancer. In the present number of this journal, Surgeon-General Sternberg describes the experiments made under his direction by a board of army surgeons in Havana. In heroic self-sacrifice and triumphant achievement these experiments have surely an absorbing interest, surpassing any fiction. Although the yellow fever parasite has not been seen, its existence seems as certain as that of the malaria parasite. We now know that yellow fever is not directly contagious, but is transmitted by a special kind of mosquito, and, probably, only in this way. If we exterminate certain kinds of mosquitoes, or prevent them from biting those diseased, or from biting those who are well, two of the most dreadful diseases—yellow fever and malaria—will be exterminated. The cost in money and life of the Spanish-American War has been more than repaid to society by the services of the medical army officers.

Science is often said to be cosmopolitan, but men of science owe allegiance to their country, and there is every reason to rejoice that it is also to an American that we owe the discovery of the probable cause of cancer. Dr. Harvey R. Gaylord, working in New York State Pathological Laboratory at Buffalo, has been able to cultivate the organisms that cause cancer, and to produce cancer by injecting them into healthy animals. These organisms are not bacteria or yeast cells, but protozoa. There has long been a difference of opinion as to whether cancer is due to alterations in nutrition, or to a parasite. Now that the latter has been proved, cancer must be regarded as a preventable disease, and it remains to discover the method of its propagation. It must, of course, be remembered that Dr. Gaylord's discovery, like all others, rests on a long line of careful researches carried on in many countries. There are innumerable names connected with the development of the germ theory of disease, but the forerunners of Gaylord, who especially deserve mention in connection with cancer, are Scheuerlin, Kubasoff, Russell, Sanfelice and Plimmer.

THE BRITISH ANTARCTIC EXPEDITION.

The resignation of Professor J. W. Gregory from the scientific staff of the British Antarctic Expedition is unfortunate, both because he possessed peculiar qualifications for his post, and because it has brought to light dissensions among those interested in the success of the expedition. The question at issue between the Royal Geographical Society, on the one hand, and the Royal Society, or some of its members, on the

other, is one frequently recurring, namely, should the executive command of scientific work be entrusted to a scientific man or is this unnecessary? When it was first arranged that Professor Gregory should take part in the expedition, it was understood that he would be the scientific leader. The British Government, however, gave a liberal subsidy, and a naval officer, Lieutenant Robert F. Scott, was appointed commander, Professor Gregory being made head of the civilian scientific staff. The relative position of Captain Scott and Professor Gregory gave rise to friction, Sir Clements Markham and the Royal Geographical Society holding that the scientific work was under the control of the naval officer in command. There were numerous conferences, and Professor Gregory finally consented to be satisfied with the control of a party to be landed on the coast. When, however, it was decided that the party should only be landed if this did not interfere with geographical exploration, Professor Gregory resigned. It seems evident that a scientific expedition can have but one leader, and it is natural that the Royal Geographical Society should regard exploration rather than geological and biological research as the primary object in the present case. The results will depend on the personality of Captain Scott, an unknown quantity in America; he may simply engage in adventure, or he may prove himself a competent scientific leader. The German expedition, with Dr. von Drygalski in absolute control, has, however, an advantage from the scientific point of view.

SCIENTIFIC NEWS.

Professor Truman Henry Safford, who since 1877 had occupied the chair of astronomy at Williams College, died on June 13, at the age of 64 years.—Dr. Otto Lugger, State entomologist of Minnesota, and well known for his important contributions to economic entomology, died of pneumonia on May 21.—John Viriamu Jones, principal of University College, South Wales, and professor of physics in that institution, died on June 2, at the age of 45 years.—The eminent paleontologist, Professor Gustaf Lindström, keeper of the department of fossil animals in the Royal Museum, Stockholm, Sweden, died on May 16.

At the annual meeting of the American Academy of Arts and Sciences, it was unanimously voted to award the Rumford Medal to Professor Elihu Thompson 'for his inventions in electric welding and lighting.'—One of the Carnegie Research Fellowships of the Iron and Steel Institute of Great Britain has been awarded to Mr. John A. Matthews, who at present holds the Columbia University Barnard Fellowship.

Dr. Frederick Peterson, of Columbia University, has been appointed by Governor Odell the medical member of the New York State Lunacy Commission. Dr. Peterson's appointment at the present time is especially fortunate, owing to the complications in connection with the State Pathological Institute, which will doubtless be settled with regard to the best interests of science and the care of the insane in the State hospitals.

DR. IRA REMSEN.

PRESIDENT OF THE JOHNS HOPKINS UNIVERSITY.

THE POPULAR SCIENCE MONTHLY.

AUGUST, 1901.

ON BODIES SMALLER THAN ATOMS.

BY PROFESSOR J. J. THOMSON,

CAMBRIDGE UNIVERSITY.

THE masses of the atoms of the various gases were first investigated about thirty years ago by methods due to Loschmidt, Johnstone Stoney and Lord Kelvin. These physicists, using the principles of the kinetic theory of gases and making certain assumptions, which it must be admitted are not entirely satisfactory, as to the shape of the atom, determined the mass of an atom of a gas; and when once the mass of an atom of one substance is known the masses of the atoms of all other substances are easily deduced by well-known chemical considerations. The results of these investigations might be thought not to leave much room for the existence of anything smaller than ordinary atoms, for they showed that in a cubic centimeter of gas at atmospheric pressure and at 0° C. there are about 20 million, million, million (2×10^{19}) molecules of gas.

Though some of the arguments used to get this result are open to question, the result itself has been confirmed by considerations of quite a different kind. Thus Lord Rayleigh has shown that this number of molecules per cubic centimeter gives about the right value for the optical opacity of the air, while a method, which I will now describe, by which we can directly measure the number of molecules in a gas leads to a result almost identical with that of Loschmidt. This method is founded on Faraday's laws of electrolysis; we deduce from these laws that the current through an electrolyte is carried by the atoms of the electrolyte, and that all these atoms carry the same charge, so that the weight of the atoms required to carry a given quantity of electricity is proportional to the quantity carried. We know

too, by the results of experiments on electrolysis, that to carry the unit charge of electricity requires a collection of atoms of hydrogen which together weigh about $^1/_{10}$ of a milligram; hence if we can measure the charge of electricity on an atom of hydrogen we see that $^1/_{10}$ of this charge will be the weight in milligrams of the atom of hydrogen. This result is for the case when electricity passes through a liquid electrolyte. I will now explain how we can measure the mass of the carriers of electricity required to convey a given charge of electricity through a rarefied gas. In this case the direct methods which are applicable to liquid electrolytes cannot be used, but there are other, if more indirect, methods, by which we can solve the problem. The first case of conduction of electricity through gases we shall consider is that of the so-called cathode rays, those streamers from the negative electrode in a vacuum tube which produce the well-known green phosphorescence on the glass of the tube. These rays are now known to consist of negatively electrified particles moving with great rapidity. Let us see how we can determine the electric charge carried by a given mass of these particles. We can do this by measuring the effect of electric and magnetic forces on the particles. If these are charged with electricity they ought to be deflected when they are acted on by an electric force. It was some time, however, before such a deflection was observed, and many attempts to obtain this deflection were unsuccessful. The want of success was due to the fact that the rapidly moving electrified particles which constitute the cathode rays make the gas through which they pass a conductor of electricity; the particles are thus as it were moving inside conducting tubes which screen them off from an external electric field; by reducing the pressure of the gas inside the tube to such an extent that there was very little gas left to conduct, I was able to get rid of this screening effect and obtain the deflection of the rays by an electrostatic field. The cathode rays are also deflected by a magnet, the force exerted on them by the magnetic field is at right angles to the magnetic force, at right angles also to the velocity of the particle and equal to $Hev \sin \theta$ where H is the magnetic force, e the charge on the particle and θ the angle between H and v. Sir George Stokes showed long ago that, if the magnetic force was at right angles to the velocity of the particle, the latter would describe a circle whose radius is mv/eH (if m is the mass of the particle); we can measure the radius of this circle and thus find m/ve. To find v let an electric force F and a magnetic force H act simultaneously on the particle, the electric and magnetic forces being both at right angles to the path of the particle and also at right angles to each other. Let us adjust these forces so that the effect of the electric force which is equal to Fe just balances that of the magnetic force which is equal to Hev; when this is the case $Fe = Hev$ or $v = F/H$. We can thus find v, and knowing

from the previous experiment the value of vm/e, we deduce the value of m/e. The value of m/e found in this way was about 10^{-7}, and other methods used by Wiechert, Kaufmann and Lenard have given results not greatly different. Since $m/e = 10^{-7}$, we see that to carry unit charge of electricity by the particles forming the cathode rays only requires a mass of these particles amounting to one ten thousandth of a milligram while to carry the same charge by hydrogen atoms would require a mass of one-tenth of a milligram.*

Thus to carry a given charge of electricity by hydrogen atoms requires a mass a thousand times greater than to carry it by the negatively electrified particles which constitute the cathode rays, and it is very significant that, while the mass of atoms required to carry a given charge through a liquid electrolyte depends upon the kind of atom, being, for example, eight times greater for oxygen than for hydrogen atoms, the mass of cathode ray particles required to carry a given charge is quite independent of the gas through which the rays travel and of the nature of the electrode from which they start.

The exceedingly small mass of these particles for a given charge compared with that of the hydrogen atoms might be due either to the mass of each of these particles being very small compared with that of a hydrogen atom or *else to the charge carried by each particle being large compared with that carried by the atom of hydrogen.* It is therefore essential that we should determine the electric charge carried by one of these particles. The problem is as follows: suppose in an enclosed space we have a number of electrified particles each carrying the same charge, it is required to find the charge on each particle. It is easy by electrical methods to determine the total quantity of electricity on the collection of particles and knowing this we can find the charge on each particle if we can count the number of particles. To count these particles the first step is to make them visible. We can do this by availing ourselves of a discovery made by C. T. R. Wilson working in the Cavendish Laboratory. Wilson has shown that when positively and negatively electrified particles are present in moist dust-free air a cloud is produced when the air is closed by a sudden expansion, though this amount of expansion would be quite insufficient to produce condensation when no electrified particles are present: the water condenses round the electrified particles, and, if these are not too numerous, each particle becomes the nucleus of a little drop of water. Now

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* Professor Schuster in 1889 was the first to apply the method of the magnetic deflection of the discharge to get a determination of the value of m/e; he found rather widely separated limiting values for this quantity and came to the conclusion that it was of the same order as in electrolytic solutions, the result of the method mentioned above as well as those of Wiechert, Kaufmann and Leonard make it very much smaller.

Sir George Stokes has shown how we can calculate the rate at which a drop of water falls through air if we know the size of the drop, and conversely we can determine the size of the drop by measuring the rate at which it falls through the air, hence by measuring the speed with which the cloud falls we can determine the volume of each little drop; the whole volume of water deposited by cooling the air can easily be calculated, and dividing the whole volume of water by the volume of one of the drops we get the number of drops, and hence the number of the electrified particles. We saw, however, that if we knew the number of particles we could get the electric charge on each particle; proceeding in this way I found that the charge carried by each particle was about 6.5×10^{-10} electrostatic units of electricity or 2.17×10^{-20} electro-magnetic units. According to the kinetic theory of gases, there are 2×10^{19} molecules in a cubic centimeter of gas at atmospheric pressure and at the temperature 0° C.; as a cubic centimeter of hydrogen weighs about 1/11 of a milligram each molecule of hydrogen weighs about $1/(22 \times 10^{19})$ milligrams and each atom therefore about $1/(44 \times 10^{19})$ milligrams and as we have seen that in the electrolysis of solutions one-tenth of a milligram carries unit charge, the atom of hydrogen will carry a charge equal to $10/(44 \times 10^{19}) = 2.27 \times 10^{-20}$ electro-magnetic units. The charge on the particles in a gas we have seen is equal to 2.17×10^{-20} units, these numbers are so nearly equal that, considering the difficulties of the experiments, we may feel sure that the charge on one of these gaseous particles is the same as that on an atom of hydrogen in electrolysis. This result has been verified in a different way by Professor Townsend, who used a method by which he found, not the absolute value of the electric charge on a particle, but the ratio of this charge to the charge on an atom of hydrogen and he found that the two charges were equal.

As the charges on the particle and the hydrogen atom are the same, the fact that the mass of these particles required to carry a given charge of electricity is only one-thousandth part of the mass of the hydrogen atoms shows that the mass of each of these particles is only about 1/1000 of that of a hydrogen atom. These particles occurred in the cathode rays inside a discharge tube, so that we have obtained from the matter inside such a tube particles having a much smaller mass than that of the atom of hydrogen, the smallest mass hitherto recognized. These negatively electrified particles, which I have called corpuscles, have the same electric charge and the same mass whatever be the nature of the gas inside the tube or whatever the nature of the electrodes; the charge and mass are invariable. They therefore form an invariable constituent of the atoms or molecules of all gases and presumably of all liquids and solids.

Nor are the corpuscles confined to the somewhat inaccessible

regions in which cathodic rays are found. I have found that they are given off by incandescent metals, by metals when illuminated by ultra-violet light, while the researches of Becquerel and Professor and Madame Curie have shown that they are given off by that wonderful substance the radio-active radium.

In fact in every case in which the transport of negative electricity through gas at a low pressure (*i. e.*, when the corpuscles have nothing to stick to) has been examined, it has been found that the carriers of the negative electricity are these corpuscles of invariable mass.

A very different state of things holds for the positive electricity. The masses of the carriers of positive electricity have been determined for the positive electrification in vacuum tubes by Wien and by Ewers, while I have measured the same thing for the positive electrification produced in a gas by an incandescent wire. The results of these experiments show a remarkable difference between the property of positive and negative electrification, for the positive electricity, instead of being associated with a constant mass 1/1000 of that of the hydrogen atom, is found to be always connected with a mass which is of the same order as that of an ordinary molecule and which, moreover, varies with the nature of the gas in which the electrification is found.

These two results, the invariability and smallness of the mass of the carriers of negative electricity, and the variability and comparatively large mass of the carriers of positive electricity, seem to me to point unmistakably to a very definite conception as to the nature of electricity. Do they not obviously suggest that negative electricity consists of these corpuscles or, to put it the other way, that these corpuscles are negative electricity, and that positive electrification consists in the absence of these corpuscles from ordinary atoms? Thus this point of view approximates very closely to the old one-fluid theory of Franklin; on that theory electricity was regarded as a fluid, and changes in the state of electrification were regarded as due to the transport of this fluid from one place to another. If we regard Franklin's electric fluid as a collection of negatively electrified corpuscles, the old one-fluid theory will, in many respects, express the results of the new. We have seen that we know a good deal about the 'electric fluid'; we know that it is molecular or rather corpuscular in character; we know the mass of each of these corpuscles and the charge of electricity carried by it; we have seen too that the velocity with which the corpuscles move can be determined without difficulty. In fact the electric fluid is much more amenable to experiment than an ordinary gas, and the details of its structure are more easily determined.

Negative electricity (*i. e.*, the electric fluid) has mass; a body negatively electrified has a greater mass than the same body in the

neutral state; positive electrification, on the other hand, since it involves the absence of corpuscles, is accompanied by a diminution in mass.

An interesting question arises as to the nature of the mass of these corpuscles which we may illustrate in the following way. When a charged corpuscle is moving, it produces in the region around it a magnetic field whose strength is proportional to the velocity of the corpuscle; now in a magnetic field there is an amount of energy proportional to the square of the strength, and thus, in this case, proportional to the square of the velocity of the corpuscle.

Thus if e is the electric charge on the corpuscle and v its velocity, there will be in the region round the corpuscle an amount of energy equal to $\frac{1}{2}\beta e^2v^2$ where β is a constant which depends upon the shape and size of the corpuscle. Again if m is the mass of the corpuscle its kinetic energy is $\frac{1}{2}mv^2$, and thus the total energy due to the moving electrified corpuscle is $\frac{1}{2}(m+\beta e^2)v^2$, so that for the same velocity it has the same kinetic energy as a non-electrified body whose mass is greater than that of the electrified body by βe^2. Thus a charged body possesses in virtue of its charge, as I showed twenty years ago, an apparent mass apart from that arising from the ordinary matter in the body. Thus in the case of these corpuscles, part of their mass is undoubtedly due to their electrification, and the question arises whether or not the whole of their mass can be accounted for in this way. I have recently made some experiments which were intended to test this point; the principle underlying these experiments was as follows: if the mass of the corpuscle is the ordinary 'mechanical' mass, then, if a rapidly moving corpuscle is brought to rest by colliding with a solid obstacle, its kinetic energy being resident in the corpuscle will be spent in heating up the molecules of the obstacle in the neighborhood of the place of collision, and we should expect the mechanical equivalent of the heat produced in the obstacle to be equal to the kinetic energy of the corpuscle. If, on the other hand, the mass of the corpuscle is 'electrical,' then the kinetic energy is not in the corpuscle itself, but in the medium around it, and, when the corpuscle is stopped, the energy travels outwards into space as a pulse confined to a thin shell traveling with the velocity of light. I suggested some time ago that this pulse forms the Röntgen rays which are produced when the corpuscles strike against an obstacle. On this view, the first effect of the collision is to produce Röntgen rays and thus, unless the obstacle against which the corpuscle strikes absorbs all these rays, the energy of the heat developed in the obstacle will be less than the energy of the corpuscle. Thus, on the view that the mass of the corpuscle is wholly or mainly electrical in its origin, we should expect the heating effect to be smaller when the corpuscles strike against a target per-

meable by the Röntgen rays given out by the tube in which the corpuscles are produced than when they strike against a target opaque to these rays. I have tested the heating effects produced in permeable and opaque targets, but have never been able to get evidence of any considerable difference between the two cases. The differences actually observed were small compared with the total effect and were sometimes in one direction and sometimes in the opposite. The experiments, therefore, tell against the view that the whole of the mass of a corpuscle is due to its electrical charge. The idea that mass in general is electrical in its origin is a fascinating one, although it has not at present been reconciled with the results of experience.

The smallness of these particles marks them out as likely to afford a very valuable means for investigating the details of molecular structure, a structure so fine that even waves of light are on far too large a scale to be suitable for its investigation, as a single wave length extends over a large number of molecules. This anticipation has been fully realized by Lenard's experiments on the obstruction offered to the passage of these corpuscles through different substances. Lenard found that this obstruction depended only upon the density of the substance and not upon its chemical composition or physical state. He found that, if he took plates of different substances of equal areas and of such thicknesses that the masses of all the plates were the same, then, no matter what the plates were made of, whether of insulators or conductors, whether of gases, liquids or solids, the resistance they offered to the passage of the corpuscles through them was the same. Now this is exactly what would happen if the atom of the chemical elements were aggregations of a large number of equal particles of equal mass; the mass of an atom being proportional to the number of these particles contained in it and the atom being a collection of such particles through the interstices between which the corpuscle might find its way. Thus a collision between a corpuscle and an atom would not be so much a collision between the corpuscle and the atom as a whole, as between a corpuscle and the individual particles of which the atom consists; and the number of collisions the corpuscle would make, and therefore the resistance it would experience, would be the same if the number of particles in unit volume were the same, whatever the nature of the atoms might be into which these particles are aggregated. The number of particles in unit volume is however fixed by the density of the substance and thus on this view the density and the density alone should fix the resistance offered by the substance to the motion of a corpuscle through it; this, however, is precisely Lenard's result, which is thus a strong confirmation of the view that the atoms of the elementary substances are made up of simpler parts all of which are alike. This and similar views of the constitu-

tion of matter have often been advocated; thus in one form of it, known as Prout's hypothesis, all the elements were supposed to be compounds of hydrogen. We know, however, that the mass of the primordial atom must be much less than that of hydrogen. Sir Norman Lockyer has advocated the composite view of the nature of the elements on spectroscopic grounds, but the view has never been more boldly stated than it was long ago by Newton who says:

"The smallest particles of matter may cohere by the strongest attraction and compose bigger particles of weaker virtue and many of these may cohere and compose bigger particles whose virtue is still weaker and so on for divers succession, until the progression ends in the biggest particles on which the operations in Chemistry and the colours of natural bodies depend and which by adhering compose bodies of a sensible magnitude."

The reasoning we used to prove that the resistance to the motion of the corpuscle depends only upon the density is only valid when the sphere of action of one of the particles on a corpuscle does not extend as far as the nearest particle. We shall show later on that the sphere of action of a particle on a corpuscle depends upon the velocity of the corpuscle, the smaller the velocity the greater being the sphere of action and that if the velocity of the corpuscle falls as low as 10^7 centimeters per second, then, from what we know of the charge on the corpuscle and the size of molecules, the sphere of action of the particle might be expected to extend further than the distance between two particles and thus for corpuscles moving with this and smaller velocities we should not expect the density law to hold.

Existence of Free Corpuscles or Negative Electricity in Metals.

In the cases hitherto described the negatively electrified corpuscles had been obtained by processes which require the bodies from which the corpuscles are liberated to be subjected to somewhat exceptional treatment. Thus in the case of the cathode rays the corpuscles were obtained by means of intense electric fields, in the case of the incandescent wire by great heat, in the case of the cold metal surface by exposing this surface to light. The question arises whether there is not to some extent, even in matter in the ordinary state and free from the action of such agencies, a spontaneous liberation of those corpuscles—a kind of dissociation of the neutral molecules of the substance into positively and negatively electrified parts, of which the latter are the negatively electrified corpuscles.

Let us consider the consequences of some such effect occurring in a metal, the atoms of the metal splitting up into negatively electrified corpuscles and positively electrified atoms and these again after a time re-combining to form neutral system. When things have got into a steady state the number of corpuscles re-combining in a given time will be equal to the number liberated in the same time. There will

thus be diffused through the metal swarms of these corpuscles, these will be moving about in all directions like the molecules of a gas and, as they can gain or lose energy by colliding with the molecule of the metal, we should expect by the kinetic theory of gases that they will acquire such an average velocity that the mean kinetic energy of a corpuscle moving about in the metal is equal to that possessed by a molecule of a gas at the temperature of the metal; this would make the average velocity of the corpuscles at 0° C. about 10^7 centimeters per second. This swarm of negatively electrified corpuscles when exposed to an electric force will be sent drifting along in the direction opposite to the force; this drifting of the corpuscles will be an electric current, so that we could in this way explain the electrical conductivity of metals.

The amount of electricity carried across unit area under a given electric force will depend upon and increase with (1) the number of free corpuscles per unit volume of the metal, (2) the freedom with which these can move under the force between the atoms of the metal; the latter will depend upon the average velocity of these corpuscles, for if they are moving with very great rapidity the electric force will have very little time to act before the corpuscle collides with an atom, and the effect produced by the electric force annulled. Thus the average velocity of drift imparted to the corpuscles by the electric field will diminish as the average velocity of translation, which is fixed by the temperature, increases. As the average velocity of translation increases with the temperature, the corpuscles will move more freely under the action of an electric force at low temperatures than at high, and thus from this cause the electrical conductivity of metals would increase as the temperature diminishes. In a paper presented to the International Congress of Physics at Paris in the autumn of last year, I described a method by which the number of corpuscles per unit volume and the velocity with which they moved under an electric force can be determined. Applying this method to the case of bismuth, it appears that at the temperature of 20° C. there are about as many corpuscles in a cubic centimeter as there are molecules in the same volume of a gas at the same temperature and at a pressure of about ¼ of an atmosphere, and that the corpuscles under an electric field of 1 volt per centimeter would travel at the rate of about 70 meters per second. Bismuth is at present the only metal for which the data necessary for the application of this method exists, but experiments are in progress at the Cavendish Laboratory which it is hoped will furnish the means for applying the method to other metals. We know enough, however, to be sure that the corpuscles in good conductors, such as gold, silver or copper, must be much more numerous than in bismuth, and that the corpuscular pres-

sure in these metals must amount to many atmospheres. These corpuscles increase the specific heat of a metal and the specific heat gives a superior limit to the number of them in the metal.

An interesting application of this theory is to the conduction of electricity through thin films of metal. Longden has recently shown that when the thickness of the film falls below a certain value, the specific resistance of the film increases rapidly as the thickness of the film diminishes. This result is readily explained by this theory of metallic conduction, for when the film gets so thin that its thickness is comparable with the mean free path of a corpuscle the number of collisions made by a corpuscle in a film will be greater than in the metal in bulk, thus the mobility of the particles in the film will be less and the electrical resistance consequently greater.

The corpuscles disseminated through the metal will do more than carry the electric current, they will also carry heat from one part to another of an unequally heated piece of metal. For if the corpuscles in one part of the metal have more kinetic energy than those in another, then, in consequence of the collisions of the corpuscles with each other and with the atoms, the kinetic energy will tend to pass from those places where it is greater to those where it is less, and in this way heat will flow from the hot to the cold parts of the metal, as the rate with which the heat is carried will increase with the number of corpuscles and with their mobility, it will be influenced by the same circumstances as the conduction of electricity, so that good conductors of electricity should also be good conductors of heat. If we calculate the ratio of the thermal to the electric conductivity on the assumption that the whole of the heat is carried by the corpuscles we obtain a value which is of the same order as that found by experiment.

Weber many years ago suggested that the electrical conductivity of metals was due to the motion through them of positively and negatively electrified particles, and this view has recently been greatly extended and developed by Riecke and by Drude, the objection to any electrolytic view of the conduction through metals is that, as in electrolysis, the transport of electricity involves the transport of matter, and no evidence of this has been detected, this objection does not apply to the theory sketched above, as on this view it is the corpuscles which carry the current, these are not atoms of the metal, but very much smaller bodies which are the same for all metals.

It may be asked if the corpuscles are disseminated through the metal and moving about in it with an average velocity of about 10^7 centimeters per second, how is it that some of them do not escape from the metal into the surrounding air? We must remember, however, that these negatively electrified corpuscles are attracted by the positively electrified atoms and in all probability by the neutral atoms as well, so

that to escape from these attractions and get free a corpuscle would have to possess a definite amount of energy, if a corpuscle had less energy than this then, even though projected away from the metal, it would fall back into it after traveling a short distance. When the metal is at a high temperature, as in the case of the incandescent wire, or when it is illuminated by ultra-violet light some of the corpuscles acquire sufficient energy to escape from the metal and produce electrification in the surrounding gas. We might expect too that, if we could charge a metal so highly with negative electricity, that the work done by the electric field on the corpuscle in a distance not greater than the sphere of action of the atoms on the corpuscles was greater than the energy required for a corpuscle to escape, then, the corpuscles would escape and negative electricity stream from the metal. In this case the discharge could be effected without the participation of the gas surrounding the metal and might even take place in an absolute vacuum, if we could produce such a thing. We have as yet no evidence of this kind of discharge, unless indeed some of the interesting results recently obtained by Earhart with very short sparks should be indications of an effect of this kind.

A very interesting case of the spontaneous emission of corpuscles is that of the radio-active substance radium discovered by M. and Madame Curie. Radium gives out negatively electrified corpuscles which are deflected by a magnet. Becquerel has determined the ratio of the mass to the charge of the radium corpuscles and finds it is the same as for the corpuscles in the cathode rays. The velocity of the radium corpuscles is, however, greater than any that has hitherto been observed for either cathode or Lenard rays: being, as Becquerel found, as much as 2×10^{10} centimeters per second or two-thirds the velocity of light. This enormous velocity explains why the corpuscles from radium are so very much more penetrating than the corpuscles from cathode or Lenard rays; the difference in this respect is very striking, for while the latter can only penetrate solids when they are beaten out into the thinnest films, the corpuscles from radium have been found by Curie to be able to penetrate a piece of glass 3 millimeters thick. To see how an increase in the velocity can increase the penetrating power, let us take as an illustration of a collision between the corpuscle and the particles of the metal the case of a charged corpuscle moving past an electrified body; a collision may be said to occur between these when the corpuscle comes so close to the charged body that its direction of motion after passing the body differs appreciably from that with which it started. A simple calculation shows that the deflection of the corpuscle will only be considerable when the kinetic energy, with which the corpuscle starts on its journey towards the charged body is not large compared with the work done

by the electric forces on the corpuscle in its journey to the shortest distance from the charged body. If d is the shortest distance, e and e' the charge of the body and corpuscles, the work done is ee'/d; while if m is the mass and v the velocity with which the corpuscle starts the kinetic energy to begin with is $\frac{1}{2}mv^2$; thus a considerable deflection of the corpuscle, *i. e.*, a collision will occur only when ee'/d is comparable with $\frac{1}{2}mv^2$; and d, the distance at which a collision occurs, will vary inversely as v^2. As d is the radius of the sphere of action for collision and as the number of collisions is proportional to the area of a section of this sphere, the number of collisions is proportional to d^2, and therefore varies inversely as v^4. This illustration explains how rapidly the number of collisions and therefore the resistance offered to the motion of the corpuscles through matter diminishes as the velocity of the corpuscles increases, so that we can understand why the rapidly moving corpuscles from radium are able to penetrate substances which are nearly impermeable to the more slowly moving corpuscles from cathode and Lenard rays.

Cosmical Effects Produced by Corpuscles.

As a very hot metal emits these corpuscles it does not seem an improbable hypothesis that they are emitted by that very hot body, the sun. Some of the consequences of this hypothesis have been developed by Paulsen, Birkeland and Arrhenius who have developed a theory of the Aurora Borealis from this point of view. Let us suppose that the sun gives out corpuscles which travel out through interplanetary space; some of these will strike the upper regions of the Earth's atmosphere and will then or even before then, come under the influnce of the Earth's magnetic field. The corpuscles when in such a field, will describe spirals round the lines of magnetic force; as the radii of these spirals will be small compared with the height of the atmosphere; we may for our present purpose suppose that they travel along the lines of the Earth's magnetic force. Thus the corpuscles which strike the earth's atmosphere near the equatorial regions where the lines of magnetic force are horizontal will travel horizontally, and will thus remain at the top of the atmosphere where the density is so small that but little luminosity is caused by the passage of the corpuscles through the gas; as the corpuscles travel into higher latitudes where the lines of magnetic force dip, they follow these lines and descend into the lower and denser parts of the atmosphere, where they produce luminosity, which on this view is the Aurora.

As Arrhenius has pointed out the intensity of the Aurora ought to be a maximum at some latitude intermediate between the pole and the equator, for, though in the equatorial regions the rain of corpuscles from the sun is greatest, the earth's magnetic force keeps these in

such highly rarefied gas that they produce but little luminosity, while at the pole, where the magnetic force would pull them straight down into the denser air, there are not nearly so many corpuscles: the maximum luminosity will therefore be somewhere between these places. Arrhenius has worked out this theory of the Aurora very completely and has shown that it affords a very satisfactory explanation of the various periodic variations to which it is subject.

As a gas becomes a conductor of electricity when corpuscles pass through it, the upper regions of the air will conduct, and when air currents occur in these regions, conducting matter will be driven across the lines of force due to the earth's magnetic field, electric currents will be induced in the air, and the magnetic force due to these currents will produce variations in the earth's magnetic field. Balfour Stewart suggested long ago that the variation on the earth's magnetic field was caused by currents in the upper regions of the atmosphere, and Schuster has shown, by the application of Gauss' method, that the seat of these variations is above the surface of the earth.

The negative charge in the earth's atmosphere will not increase indefinitely in consequence of the stream of negatively electrified corpuscles coming into it from the sun, for as soon as it gets negatively electrified it begins to repel negatively electrified corpuscles from the ionized gas in the upper regions of the air, and a state of equilibrium will be reached when the earth has such a negative charge that the corpuscles driven by it from the upper regions of the atmosphere are equal in number to those reaching the earth from the sun. Thus, on this view, interplanetary space is thronged with corpuscular traffic, rapidly moving corpuscles coming out from the sun while more slowly moving ones stream into it.

In the case of a planet which, like the moon, has no atmosphere there will be no gas for the corpuscles to ionize, and the negative electrification will increase until it is so intense that the repulsion exerted by it on the corpuscles is great enough to prevent them from reaching the surface of the planet.

Arrhenius has suggested that the luminosity of nebulæ may not be due to high temperature, but may be produced by the passage through their outer regions of the corpuscles wandering about in space, the gas in the nebulæ being quite cold. This view seems in some respects to have advantages over that which supposes the nebulæ to be at very high temperatures. These and other illustrations, which might be given did space permit, seem to render it probable that these corpuscles may play an important part in cosmical as well as in terrestrial physics.

DR. WILLIAM GILBERT.

THE PHILOSOPHER OF COLCHESTER.

GILBERT OF COLCHESTER.

THE TERCENTENARY OF ELECTRIC AND MAGNETIC SCIENCE.

BY BROTHER POTAMIAN, D.SC., LOND.,
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AT a period when natural science was taught and studied in the schools of Europe from text-books, we find Gilbert of Colchester proclaiming by example and advocacy the paramount value of experiment for the advancement of learning. He was unsparing in his denunciation of the superficiality and verbosity of mere bookmen, and had no patience with writers who treated their subjects 'esoterically, reconditely and mystically.' For him, the laboratory method was the only one that could secure fruitful results and effectively push back the frontiers of knowledge.

It is true that men of unusual ability and strong character strove before his time to adjust the claims of authority in matters scientific. While respectful of the teachings of recognized leaders, they were not awed into acquiescence by the customary academical 'magister dixit.' On the contrary, they wanted to test with their eyes in order to judge with reason; believing in the supreme importance of experiment, they sought to acquire a knowledge of nature from nature herself.

Such were Albert the Great and Friar Bacon. Albert did not bow obsequiously to the authority of Aristotle or any of his Arabian commentators; he investigated for himself and became, for his age, a distinguished botanist and physiologist.

Roger Bacon, after absorbing the learning of Oxford and Paris, wrote to the reigning Pontiff, Clement IV., urging him to have the works of the Stagyrite burnt in order to stop the propagation of error in the schools. The Franciscan monk of Ilchester has left us in his *Opus Majus* a lasting memorial of his practical genius. In the section entitled 'Scientia Experimentalis,' he affirms that "Without experiment, nothing can be adequately known. An argument proves theoretically but does not give the certitude necessary to remove all doubt, nor will the mind repose in the clear view of truth, unless it find it by way of experiment." And in his *Opus Tertium:* "The strongest arguments prove nothing so long as the conclusions are not verified by experience. Experimental science is the queen of sciences and the goal of all speculation."

No one, even in our own times, ever wrote more strongly in favor of the practical method than did this follower of St. Francis in the thirteenth century. Being convinced that there can be no conflict between scientific and revealed truths, he became an irrepressible advocate for observation and experiment in the study of the phenomena and forces of nature.

The example of Albert and of Friar Bacon, not to mention others like Vincent of Beauvais, the Dominican encyclopedist, was, however, not sufficient to wean students and professors from the easy-going routine of book-learning. A few centuries had to elapse before the weaning was effectively begun; and the man who powerfully contributed to this result was Dr. William Gilbert, the philosopher of Colchester.

Gilbert was born in Colchester * in 1540 in a house which, thanks to the appreciation of the authorities of that ancient town, the writer found in an excellent state of preservation on the occasion of his visit in quest of Gilbertiana.

Having received the elements of his education in the grammar school of his native town, Gilbert entered St. John's College, Cambridge. He graduated in 1560, 'commenced' M. A. in 1564, and took his M. D. degree in 1569. On leaving the university, he traveled for some time on the Continent, where he made the acquaintance of several distinguished scholars. On his return to England he practised, we are told, 'with great success and applause.' His reputation obtained for him the presidency of the Royal College of Physicians for the year 1600, and ultimately led to his being appointed physician in ordinary to the Queen (Elizabeth).

We are not here concerned with Gilbert as a physician and still less as a courtier. His claims to enduring recognition are of a higher order, for we regard him not only as the author of a monumental work of physical research, but also as founder, by word and deed, of the Experimental School of philosophy.

In his address to the 'candid reader' at the beginning of *De Magnete*† he pointedly says:

> "To you alone, true philosophers, ingenuous minds, who not only in books, but in things themselves, look for knowledge, have I dedicated a new style of philosophizing. But if any of you see fit not to agree with the opinions expressed, let them note the great multitude of experiments and discoveries, for it is these that cause all philosophy to flourish: we have dug them up and demonstrated them with much pains and sleepless nights and great money expense."

Further on he adds:

> "Nor did we find this, our labor, vain and fruitless, for every day in our experiments novel and unheard of properties came to light."

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* A town in Essex, fifty miles northeast of London.

† The only English translation of this work which we have is by Mr. P. Henry Mottelay, of New York, published by Wiley & Sons.

On one occasion, he hears that Baptista Porta, whom he calls 'a philosopher of no ordinary note,' said that a piece of iron rubbed with a diamond turns to the north. He suspects this to be heresy. So, forthwith he proceeds to test the statement by experiment. He was not dazzled by the reputation of Baptista Porta: he respected Porta, but respected truth even more. He tells us that he experimented with seventy-five diamonds in presence of many witnesses, employing a number of iron bars and pieces of wire, manipulating them with the greatest care while they floated on corks; and he concludes his long and exhaustive research by plaintively saying: "Yet never was it granted me to see the effect mentioned by Porta."

Though it led to a negative result, this probing inquiry was a masterpiece of experimental work.

Gilbert incidentally regrets that the men of his time "are deplorably ignorant with respect to natural things," and the only way he sees to remedy this is to make them "quit the sort of learning that comes only from books and that rests only on vain arguments and conjectures," for he shrewdly remarks that "even men of acute intelligence without actual knowledge of facts and in the absence of experiment easily fall into error."

Acting on this intimate conviction, he labored for eighteen years over the theories and experiments which he sets forth in his great work on the magnet. "There is naught in these books," he tells us, "that has not been investigated, and again and again done and repeated under our eyes." He begs any one that should feel disposed to challenge his results to repeat the experiments for himself "carefully, skilfully and deftly, but not heedlessly and bunglingly."

It has often been said that we are indebted to Sir Francis Bacon, Queen Elizabeth's Chancellor, for the inductive method of studying the phenomena of nature. This is an error, for all investigators employed it from Archimedes the Syracusan down to Gilbert, the contemporary of Lord Verulam.* Bacon's merit lies in the fact that he not only minutely analyzed the method, pointing out its uses and abuses, but also that he admitted it to be the only one by which we can attain an accurate knowledge of the physical world around us. His sententious eulogy went forth to the world of scholars invested with all the importance, authority and dignity which the high position and world-wide fame of the philosophic Chancellor could give it. But while Bacon thought and wrote in his study, Gilbert labored and toiled in his workshop. By his quill, Bacon made a profound impression on the philosophic mind of his age; by his researches, Gilbert explored two

* Bacon was raised to the peerage as Baron Verulam, and was subsequently created Viscount St. Albans; where, then, is the propriety of calling him Lord Bacon?

vast provinces of nature and added them to the domain of science. Bacon was a theorist, Gilbert an investigator. For eighteen years and more he shunned the glare of society and the throbbing excitement of public life; he wrenched himself away from all but the strictest exigencies of his profession, in order to devote himself undistractedly to the pursuit of science. And all this more than twenty years before the appearance of Bacon's *Novum Organum*, the very work which contains the philosopher's 'large thoughts and lofty phrases' on the value of experiment as a means for the advancement of learning. During that long period Gilbert haunted Colchester, where he delved into the secrets of nature and prepared the materials for his grand work, *De Magnete*. The publication of this Latin treatise made him known in the universities at home and especially abroad: he was appreciated by all the great physicists and mathematicians of his age; by such men as Sir Kenelm Digby; by William Barlowe, a great 'magneticall' man; by Kepler, the astronomer, who adopted and defended his views; by Galileo himself, who said: 'I extremely admire and envy the author of *De Magnete*.'

If any one then deserves to be called the founder of the experimental school of philosophy, we contend that it is not Bacon the thinker and essayist, but Gilbert the methodical worker and fruitful discoverer.

The originality of Gilbert's work and the character of his discoveries, together with the reputation which he enjoyed in the greater seats of learning, ended by giving umbrage to Bacon, and the world saw the strange spectacle of a great chancellor forgetting the teachings of his own philosophy and becoming jealous. He even carried his ill feeling so far as to write belittlingly of the conclusions of his illustrious rival, saying in his *De Augmentis Scientiarum* that "Gilbert had attempted a general system upon the magnet, endeavoring to build a ship out of materials not sufficient to make the rowing-pins of a boat."

It will be interesting to see what these 'rowing-pins' were, for then we shall have a scale by which to judge the intensity of Bacon's jealousy and the magnitude of his belittling ability.

II.

Gilbert's electrical work is contained in Book II of *De Magnete*; and we may say at once that the second chapter of that famous book is the first chapter on electricity ever written. Nothing was known before Gilbert's time save the attraction for light bodies observed in rubbed amber and jet.

Gilbert goes to work and devises an instrument to enable him to study readily the electrical behavior of rubbed substances. He called it a Versorium, we should call it an electroscope. "Make to yourself," he says, "a rotating needle of any sort of metal three or four fingers long and pretty light and poised on a sharp point." He then briskly

rubs and brings near his versorium glass, sulphur, opal, diamond, sapphire, carbuncle, rock-crystal, sealing-wax, alum, resin, etc., and he finds that all these attract his suspended needle, and not only the needle, but everything else. His words are remarkable: 'Ad electrica feruntur omnia.'* Here is a great advance on the amber and jet, the only two bodies previously known as having the power to attract 'straws, chaff and twigs,' the usual test-substances of the ancients. Pursuing his investigations, he finds a class of bodies which perplex him, because when rubbed they do not affect his electroscope. Among these he enumerates: bone, ivory, marble, flint, silver, copper, gold, iron, even the lodestone itself. The former class he called *electrica,* electrics, deriving the term from *elektron,* the Greek name for amber; the latter class he termed *anelectrica,* non-electrics.

Science therefore owes to Gilbert the terms electric and electrical; the term electricity was a coinage of a later period, due probably to the illustrious Irish philosopher, Robert Boyle, who uses the term in his work *On the Mechanical Production of Electricity,* published in 1675.† Gilbert never uses *electricitas,* but speaks of corpora electrica, effluvia electrica, attractio electrica, motus electricus and the like. Had Gilbert chosen the Latin name for amber, *succinum,* as he might have done, we should not be speaking to-day of electricity, electrostatics, electro-optics, electrics, dielectrics; but should probably be using succinic for electric, succinical for electrical, succinicity for electricity, together with a series of harsh-sounding derivatives and compounds.

As we said, Gilbert was perplexed by the anomalous behavior of his anelectrics. He toiled and labored hard to find out the cause. He undertook a long, abstract, philosophical discussion of the nature of bodies which, from its very subtlety, failed to reveal the cause of his perplexing anomaly. Gilbert failed to discover the distinction between conductors and insulators, and, as a consequence, never found out that similarly electrified bodies repel each other. Had he but suspended an excited stick of sealing-wax, what a promised land of electrical wonders would have unfolded itself to his vision! and what a harvest of results such a reaper would have gathered in! He noticed the effect of distance; for he says, 'The nearer the electric is to the versorium, the quicker is the attraction.' It was reserved, however, for the French mathematician and engineer, Coulomb, to show in 1785 that the law of attraction or repulsion between two electrified particles varies inversely as the square of the distance between them.

From solids, he proceeds to examine the behavior of some liquids, and finds that they too are susceptible of electrical influence. He notices that a piece of excited amber when brought near a drop of water

* All things are drawn to electrics.

† To Boyle we are also indebted for the name barometer.

deforms it, drawing it out into a conical shape. He even experiments with smoke, concluding that the small carbon particles are attracted by an electrified body. It was only a few years ago that Dr. Oliver Lodge, extending this observation, proposed to lay the poisonous dust floating about in the atmosphere of lead works by means of large electrostatic machines. He even hinted in his Royal Institution lecture that they might be useful in dissipating mists and fogs, recommending that a trial be made on some of our ocean-bound steamers.

Gilbert next tries heat as an agent to produce electrification. He takes a red-hot coal and finds that it has no effect on his electroscope; he heats a mass of iron up to whiteness and finds that it too exerts no electrical effect. He tries a flame, a candle, a burning torch, and concludes that all bodies are attracted by electrics save those that are afire or flaming, or extremely rarefied. He then reverses the experiment and brings near an excited body the flame of a lamp, and he ingenuously states that the body no longer attracts the pivoted needle. He thus discovered the neutralizing effect of flames, and supplied us with the readiest means we have to-day of discharging non-conductors.

He goes a step further; for we find him exposing some of his electrics to the action of the sun's rays in order to see whether they acquired a charge; but all his results were negative. He then concentrates the rays of the sun by means of lenses, evidently expecting some electrical effect; but finding none, he concludes with a vein of pathos that the sun imparts no power, but dissipates and spoils the electric effluvium.

Professor Righi has shown that a clean metallic plate acquires a positive charge when exposed to the ultra-violet radiation from any artificial source of light, but that it does not when exposed to solar rays. The absence of electrical effects in the latter case is attributed to the absorptive action of the atmosphere on the shorter waves of the solar beam.

Of course, Gilbert permits himself some speculation as to the nature of the agent he was dealing with. He thought of it, reasoned about it, pursued it in every way; and came to the conclusion that it must be something extremely tenuous indeed, but yet substantial, ponderable, material. "As air is the effluvium of the earth," he says, "so electrified bodies have an effluvium of their own, which they emit when stimulated or excited;" and again: "It is probable that amber exhales something peculiar that attracts the bodies themselves."

In 1862, another Gilbert, Sir Wm. Thomson (now Lord Kelvin), writing to his friend, Professor Tait, of Edinburgh, said: "Tell me what electricity is and I'll tell you everything else"; and in April, 1893, the same Lord Kelvin, replying to the writer, added: "I see no reason to say otherwise than what you tell me I said to Professor Tait in 1862." Despite, then, the great work of Clerk Maxwell and the corroborative

experiments of Hertz, we must still admit that the ultimate nature of electricity remains wrapped in mystery. It is true, we discard the material effluvium of Gilbert, but only to substitute for it an ethereal ripple, a quiver, a wave motion in the hypothetical ether with which we fill all space.

From 1580 to 1600, we find Gilbert spending in his workship all the leisure he can snatch from his professional duties. He notes down his experiments, his failures as well as his successes, discusses them, reasons on them, and pursues his inquiry further and further. In a word, we find him toiling in his workshop at Colchester as Faraday toiled more than 200 years later in the low, dark rooms of the Royal Institution of Great Britain. Both were actuated by the same calm, persevering, experimental spirit. Gilbert founded and christened the science of electrics: he left it in its infancy, it is true; but with sufficient vitality to enable it to survive the neglect of years, until at last it was taken up and fondly cared for by our Franklins and Faradays.

III.

The science of magnetism is even more indebted to Gilbert than that of electricity. The ancients spoke of the lodestone as the Magnesian stone, from its being found in Magnesia, in Asia Minor. Gilbert constantly uses the adjective *magnetica;* and it is to his use of that word that we owe the terms magnet, magnetic and magnetism.* He showed that a great number of bodies could be electrified: but he maintained that those only could exhibit magnetic properties which contain iron. He satisfies himself of this by rubbing with a lodestone such substances as wood, gold, silver, copper, zinc, lead, glass, etc., and then floating them on corks, quaintly adding that they show 'no poles, because the energy of the lodestone has no entrance into their interior.'

To-day we know that nickel and cobalt behave like iron, whilst antimony, bismuth, copper, silver and gold are susceptible of being influenced by powerful electromagnets, showing what has been termed diamagnetic phenomena. Even liquids and gases, in Faraday's classical experiments, yielded to the influence of his great magnet; and Professor Dewar, in the same Royal Institution, exposed some of his liquid air and liquid oxygen in the presence of the writer to the influence of Faraday's electromagnet and found them to be strongly attracted, thus behaving like the paramagnetic bodies, iron, nickel and cobalt.

Gilbert observes in all his magnets two points, one near each end, in which the force, or, as he terms it, 'the supreme attractional power,' is concentrated. He terms these *poles* by analogy to the earth; and he will have it understood that these poles are not mathematical points, as

* According to Humboldt, "the barbarous word *magnetismus*" was introduced in the eighteenth century.

the attraction manifests itself 'all over the periphery.' Following the same analogy, he calls the line joining the two poles the axis of the magnet, and the equator the line equally distant from them. With the aid of his steel versorium, he recognizes that similar poles are mutually hostile, whilst opposite poles seize and hold each other in friendly embrace. He also satisfies himself that the energy of magnets resides not only in their extremities, but that it permeates 'their inmost parts, being entire in the whole and entire in each part.' This is exactly what we say; it is nothing else than the molecular theory proposed by Weber, extended by Ewing and universally accepted.

At any rate, Gilbert is quite certain that whatever magnetism may be, it is not, like electricity, a material, ponderable substance; he ascertained this by weighing in the most accurate scales of a goldsmith a rod of iron before and after it had been rubbed with the lodestone, and then observing that the weight is precisely the same in both cases, being 'neither less nor more.' He discovers also that not only the magnet, but all the space surrounding it, possesses magnetic properties; for the magnet 'sends its force abroad in all directions, according to its energy and quality.' This region of influence he calls *orbis virtutis*, a sphere, or, as we call it, a field of force. With wonderful intuition, he sees this space filled with lines of magnetic virtue passing out radially from his spherical lodestone, and he calls these lines *radii virtutis magneticae,* rays of magnetic force.

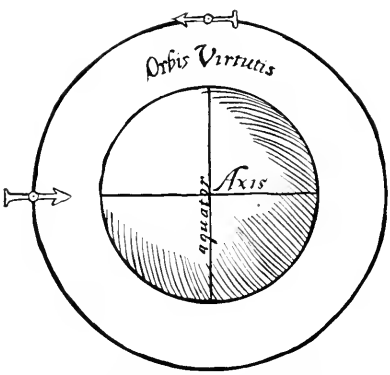

FIG. 1. GILBERT'S SPHERICAL LODESTONE AND FIELD OF MAGNETIC FORCE.

When Faraday spoke of field of force, magnetic field, lines of electric and magnetic induction, some thought the idea new, whereas not only the idea, but also the very terms occur with appropriate illustrations in De *Magnete.*

Clerk Maxwell was so fascinated with that beautiful concept that he made it the work of his life to study the field of force due to electrified bodies, to magnets and to conductors conveying currents; his powerful intellect visualized those lines and gave them accurate mathematical expression in the great treatise on electricity and magnetism which he gave to the world in 1873.

Gilbert observes that the lodestone may be spherical or oblong; 'whatever the shape, imperfect or irregular, verticity is present, there are poles,' and the lodestones 'have the selfsame way of turning to the poles of the world.' We find Gilbert working even with a ring of iron. He

strokes it with a natural magnet and feels certain that he has magnetized it; and he assures us that 'one of the poles will be at the point rubbed and the other will be at the opposite side;' and how does he convince himself that the ring is really magnetized? He cuts it across at the point opposite the one rubbed, opens it out, and finds that the ends exhibit polar properties.

A favorite piece of apparatus with Gilbert was a lodestone ground down into globular form. He called it a terrella, a miniature earth. He used it extensively for reproducing the phenomena described by magnetizers, travelers and navigators as observed in their compass needles. He breaks up terrellas, in order to examine the magnetic condition of their inner parts. There is not a doubtful utterance in his description of what he finds; he speaks clearly and emphatically. "If magnetic bodies be divided, or in any way broken up, each several part hath a north and a south end;" *i. e.*, each part will be a complete magnet.

We find him also comparing magnets by what is known to us as the 'magnetometer method.' He brings the magnetized bars in turn near a compass needle and concludes that the magnet or the lodestone which is able to make the needle go round is the best and strongest. He also seeks to compare magnets by a process of weighing, similar to what is called, in laboratory parlance, the 'test-nail' method. He also inquires into the effect of heat upon his magnets, and finds that 'a lodestone subjected to any great heat loses some of its energy.' He applies a red-hot iron to a compass needle and notices that it 'stands still, not turning to the iron.' He thrusts a magnetized bar into the fire until it is red-hot and shows that it has lost all magnetic power. He does not stop at this remarkable discovery, for he proceeds to let his red-hot bars cool while lying in various positions, and he finds: (1) that the bar will acquire magnetic properties if it lie in the magnetic meridian; and (2) that it will acquire none if it lie east and west. These effects he rightly attributes to the inductive action of the earth.

Gilbert marks these and other experiments with marginal asterisks; small stars denoting minor and large ones important discoveries of his. There are in all 21 large and 178 small asterisks, as well as 84 illustrations in *De Magnete*. This implies a vast amount of original work, and forms no small contribution to the foundations of electric and magnetic science.

Gilbert clearly realized the phenomena and laws of magnetic induction. He tells us that "as soon as a bar of iron comes within the lodestone's sphere of influence, though it be at some distance from the lodestone itself, the iron changes instantly and has its form renewed; it was before dormant and inert: but now is quick and active." He hangs a nail from a lodestone; a second from the first, a third from the

second and so on—a well-known experiment, made every day for elementary classes. Nor is this all, for he interposes between his lodestone and a mass of iron thick boards, walls of pottery and marble, and even metals, and he finds that there is naught so solid as to do away with this force or to check it, save a plate of iron. All that can be added to this pregnant observation is that the plate of iron must be very thick in order to carry all the lines of force due to the magnet, and thus completely screen the space beyond.

But Gilbert is astonishing when he goes on to make thick boxes of gold, glass and marble, and suspending his needle within them, declares with excusable enthusiasm that regardless of the box which imprisons

FIG. 2. GILBERT HEATED AND HAMMERED BARS OF IRON IN THE MAGNETIC MERIDIAN, AND ALLOWED THEM TO COOL WHILE LYING NORTH (*Septentrio*) AND SOUTH (*Auster*).

the magnet, it turns to its predestined points of north and south. He even constructs a box of iron, places his magnet within, observes its behavior, and concludes that it turns north and south, and would do so were 'it shut up in iron vaults sufficiently roomy.' Our experiments show that if the sides of the box are thin, the needle will experience the directive force of the earth; but if they are sufficiently thick—thick as the walls of an ordinary safe—the inside of such a box will be completely screened; none of the earth's magnetic lines will get into it so that the needle will remain indifferently in any position in which it is placed. A few years ago, the physical laboratory of St. John's College, Oxford, was screened from the obtrusive lines of neighboring dynamos

by building two brick walls parallel to each other and eight inches apart and filling in the space with scrap iron. A delicate magnetometer showed that this structure allowed no leakage of lines of force through it, but offered an impenetrable barrier to the magnetic influence of the working dynamos.

Gilbert's greatest discovery is that the earth itself is a vast globular magnet. 'Magnus magnes ipse est globus terrestris' are his own words. It has its poles, its axis and equator just as the lodestone or terrella. The pole in our hemisphere he variously calls north, boreal, arctic, whilst that in the other hemisphere he calls south, austral, antarctic. He is quite aware that his theory is a grand generalization; and admits that it is 'a new and till now unheard-of view,' and so confident is he in its worth that he is not afraid to say that 'it will stand as firm as aught that ever was produced in philosophy, backed by ingenious argumentation or buttressed by mathematical demonstration.' Three hundred years have passed away, and Gilbert's theory is accepted by every man of science and is taught in every school of physics. Moreover, save the correction of a few errors of observation, no change of any importance has been made in it.

Gilbert sought to explain the magnetic condition of our globe by the presence, especially in its innermost parts, of what he calls true terrene matter, homogeneous in structure and endowed with magnetic properties, so that every separate fragment of the earth exhibits the whole force of magnetic matter. We attribute terrestrial magnetism to the vast masses of magnetic material which lie near the surface, for at a depth of ten or twelve miles the temperature of the ferruginous masses would deprive them of all magnetic properties. The magnetic condition of the earth is also attributed to the action of electric currents continually flowing through the crust of the earth. Both these theories, as Professor Rücker, of London, said in 1891, are beset with difficulties; at present we must content ourselves with accumulating facts in the hope that a clue to an explanation may hereafter be found.

Gilbert's discovery enabled him to offer a philosophical explanation of the behavior of both compass and dipping needles, as well as of a great many other phenomena. The declination was known before Gilbert's time. Columbus noticed this want of coincidence between the geographical and magnetic meridians in his first voyage to the New World. It was on September 13, 1492, when 200 leagues west of Teneriffe, that his attentive eye observed that the magnet pointed slightly west of north, and that this angular deviation increased during the following days.* For a time he kept the secret in his own mind; the pilots, however, soon perceived the variations and grew alarmed, deserted, as

* Columbus was thus the first to observe that the declination or 'variation of the compass,' as it is called, changes with place.

they said they were, on the trackless ocean by their only trusty guide. Columbus calmed their fears by saying that the needle did not turn to the polar star, but to some fixed and invariable point near it. This explanation, born of inspiration, quieted the sailors, who marveled much at the Admiral's great astronomical knowledge. Gilbert rightly states that the declination changes with place, but he slips into error when he says: "As the needle hath ever inclined towards east or towards west, so even now does the arc of variation continue to be the same in whatever place or region, be it sea or continent; so, too, will it be forever unchanging."

We know, however, that for any given place this angle is continuously, though slowly, changing. Some of these changes require centuries for the completion of their cycle, and are therefore called 'secular'; others require but a year, and are termed 'annual'; whilst others run their course in the space of a day and are known as 'diurnal.' Though these periodic changes in the declination have been established by careful and prolonged observations, we can not say that they are yet satisfactorily accounted for.

The dip of the needle was also familiar to Gilbert, having been first observed in 1576 by Robert Norman. Our philosopher illustrates this phenomenon by balancing a piece of steel so that it remains exactly horizontal when unmagnetized, and by observing that the moment it is stroked by a magnet its north-seeking end dips 'as low as the fulcrum on which it is supported permits.' Gilbert moves a needle over his terella, and finds (1) that the dip is 0° on the equator, (2) that it gradually increases with the latitude being 90° at either pole.

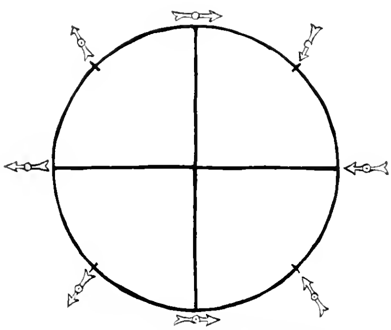

Fig. 3. Gilbert's Terella showing the Behavior of a Dipping-Needle at its Poles, Equator and other Intervening Places.

He extends this experiment from the terrella to the earth itself, and even devises an instrument for determining the latitude of any place on land or on sea in the thickest weather and in the darkest night, 'without the help of sunne, moone, or starre.' In this, however, he was wrong. For he assumed the isoclinic lines to be circles running parallel to the magnetic equator, which he erroneously supposed to coincide with the geogrpahical equator.

Gilbert recognized that the earth exerts on a freely movable needle a force that gives it direction and not a motion of translation. He illustrates this by floating a needle on a cork and observing that it points

N. and S., remaining all the while at any place in the vessel of water in which it may be put. His words, though quaint, are exact: "It revolves on its iron center, and is not borne towards the rim of the vessel." He knew nothing of the mechanical couple in play; but he knew the fact; and with the instinct of a true philosopher, tested it in a variety of ways. With a most luminous insight into terrestrial magnetic phenomena, he observed that near the poles a compass needle tending, as it does, to dip greatly, must necessarily experience only a feeble horizontal directive force. To this he adds that 'at the poles there is no direction,' meaning thereby that a properly balanced compass needle

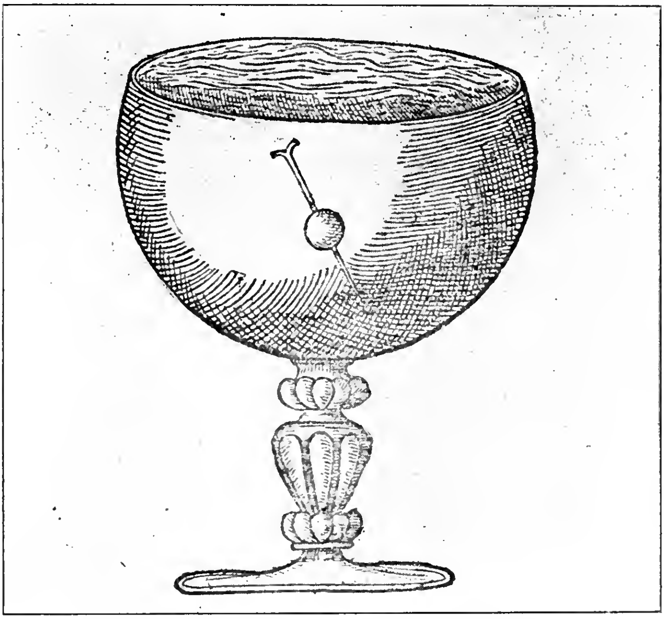

FIG. 4. GILBERT'S METHOD OF SHOWING MAGNETIC DIP.

would remain indifferently in any azimuth in which it might be placed. We express the same by saying that the horizontal component of the earth's force vanishes at the poles.

Gilbert dwells at length on the inductive action of the earth. We have seen him hammering heated bars of iron and then allowing them to cool while lying in the magnetic meridian. He notes that they become magnetized, and does not fail to point out the polarity of each end. He likewise attributes to the influence of the earth the magnetic condition acquired by iron bars that have for a long time lain fixed in the north and south position as bars often are fixed in buildings and in windows, and he ingenuously adds: for great is the effect of long-con-

tinued direction of a body towards the poles. To the same cause he attributes the magnetization of iron crosses fixed to steeples, towers, etc.

It must be evident from this brief analysis of *De Magnete*,

1. That Gilbert was acquainted with all the facts in magnetism known in his days;

2. That he added profusely to the number;

3. That he coordinated these facts and deduced the laws which govern them; and

4. That he was the first to offer a scientific explanation of the behavior of the compass and the dip needle, as well as of numerous other phenomena, correctly attributed by him to the magnetic state of our globe.

Such were some of the 'rowing pins,' as Chancellor Bacon ironically calls them, with which Gilbert built up one of the greatest monuments ever erected by the genius of one man. Had Gilbert done nothing else than propound and establish on the solid basis of observation and experiment his theory that the earth is a great magnet, his name would ever live in the annals of science, surrounded with a halo that even the unjust strictures of Bacon could not dim; but when we consider his spirited advocacy of research at the end of the sixteenth century, and the cardinal advances he achieved in the interpretation of two great branches of knowledge, we can have no hesitation in considering him with Poggendorff, 'the Galileo of Magnetism,' and with Priestley, 'the Founder of Modern Electricity.'

Were we asked to write an inscription for his statue, we should write the simple words:

Gilbert, the Columbus of the Electrical World.

THE PEOPLING OF THE PHILIPPINES.

BY PROFESSOR RUD. VIRCHOW.

PART II.*

When, on the 18th of March, 1897, I made a communication on the population of the Philippines, a bloody uprising had broken out everywhere against the existing Spanish rule. In this uprising a certain portion of the population, and indeed that which had the most valid claim to aboriginality, the so-called Negritos, was not involved. Their isolation, their lack of every sort of political, often indeed of village organization, also their meager numbers, render it conceivable that the greatest changes might go on among their neighbors without their taking such a practical view of them as to lead to their engaging in them. Thus it can be understood how they would take no interest in the further development of the affair.

Since then the result of the war between Spain and the United States has been the destruction of Spanish power, and the treaty of Paris brought the entire Philippine Archipelago into the possession of the United States of America. Henceforth the principal interest is centered upon the deportment of the insurgents, who have not only outlived the great war between the powers, but are now determined to assert, or win, their independence from the conquerors. These insurgents, who for brevity are called Filipinos, belong, as I have remarked, to the light-colored race of so-called Indios, who are sharply differentiated from the Negritos. Their ethnological position is difficult to fix, since numerous mixtures have taken place with immigrant whites, especially with Spaniards, but also with people of yellow and of brown races—that is, with Mongols and Chinese.† Perhaps here and there the importance of this mixture on the composite type of the Indios has been overestimated: at least in most places positive proof is not forthcoming that foreign blood has imposed itself upon the bright-colored population. Both history and tradition teach, on the contrary, as also the study of the physical peculiarities of the people, that among the various tribes differences exist which suggest family traits. To this effect is the testimony of several travelers who have followed one

* Sitzungsberichte der Königlichen Preussischen Akademie der Wissenschaften zu Berlin. Berlin, 1899, Vol. III, 19th January, pp. 14-26.

† NOTE.—A brief résumé of these many mixtures is given in Tour du Monde, 27th May and 3d June, 1882; see also statement in this translation. —TRANSLATOR.

another during a long period of time, as has been developed especially by Blumentritt.

In this connection it must not be overlooked that all these immigrations, howsoever many they be supposed to have been, must have come this way from the west. Indeed, a noteworthy migration from the east is entirely barred out, if we look no farther back than the Chinese and Japanese. On the contrary, all signs point to the assumption that from of old, long before the coming of Portuguese and Spaniards, a strong movement had gone on from this region to the east, and that the great sea way which exists between Mindanao and the Sulu islands on the north and Halmahera and the Moluccas on the south was the entrance road along which those tribes, or at least those navigators whose arrival peopled the Polynesian Islands, found their way into the Pacific Ocean. But also the movement of the Polynesians points to the west, and if their ancestors may have come from Indonesia there is no doubt that in their long journeys eastward they must have touched at the coasts of other islands on their way, especially the Philippines. Polynesian invasions of the Philippines are not supposed to have closed when a migration of peoples or of men passing out to the Pacific Ocean laid the foundation of a large fraction of the population of the archipelago. It is known that now and then single canoes from the Palawan or the Ladrone islands were driven upon the east coast of Luzon, but their importance ought not to be overestimated. The migration this way from the west must henceforth remain as the point of departure for all explanations of this eastern ethnology.

[These statements are well enough for working hypotheses, but actual proofs are not at hand. Ratzel, Berl. Verhandl., etc., Phil. Hist. Class, 1898, I, p. 33.—TRANSLATOR.]

Now, how are the local differences of various tribes to be explained, when on the whole the place of origin was the same? Is there here a secondary variation of the type, something brought about through climate, food, circumstances? It is a large theme, which, unfortunately, is too often dominated by previously-formed theories. The importance of 'environment' and mode of life upon the corporeal development of man can not be contested, but the measure of this importance is very much in doubt. Nowhere is this measure, at least in the present consideration, less known than in the Philippines. In spite of wide geological and biological differences on these islands, there exists a close anthropological agreement of the Indios in the chief characteristics, and the effort to trace back the tribal differences that have been marked to climatic and alimentary causes has not succeeded. The influence of inherited peculiarities is also more mighty here, as in most parts of the earth, than that of 'milieu.'

If we assume, first, that the immigrants brought their peculiarities with them, which were fixed already when they came, we must also accept as self-evident that the Negritos of the Philippines do not belong to the same stock as the more powerful, bright-colored Indios. As long as these islands have been known, more than three centuries, the skin of the Negritos has been dark brown, almost black, their hair short and spirally twisted, and just as long has the skin of the Indios been brownish, in various shades, relatively clear, and the hair has been long and arranged in wavy locks. At no time, so far as known, has it been discovered that among a single family a pronounced variation from these peculiarities had taken place. On this point there is entire unanimity. In case of the Negritos there is not the least doubt; of the Indios a doubt may arise, for, in fact, the shades of skin color appear greatly varied, since the brown is at times quite blackish, at times yellowish, almost as varied as is the color of the sunburnt hair. But even then the practised eye easily detects the descent, and if the skin alone is not sufficient the first glance at the hair completes the diagnosis. The correct explanation of individual or tribal variations is difficult only with the Indios, while no such necessity exists in the case of the Negritos. But among the Indios these individual and tribal variations are so frequent and so outspoken that one is justified in making the inquiry whether there has not developed here a new type of inherited peculiarities. If this were the case, it must still be held that already the immigrant tribes had possessed them.

Now, history records that different immigrations have actually taken place. Laying aside the latest before the arrival of the Spaniards, that of the Islamites, in the fourteenth and fifteenth centuries, there remains the older one. If ethnologists and travelers in general come to the conclusion concerning Borneo—and it is to be taken as certain—that the differences now existing among the wild tribes of this island are very old, it ought not be thought so wonderful if, according to the conditions of the tribes which have immigrated thence, there should exist on the Philippines near one another dissimilar though related peoples. This difference is not difficult to recognize in manners and customs—a side of the discussion which is further on to be treated more fully. We begin with physical characteristics.

Among these the hair occupies the chief place. To be sure, among all the Indios it is black, but it shows not the slightest approach to the frizzled condition which is such a prominent feature in the external appearance of the Negritos and of all the Papuan tribes of the East. This frizzled condition may be called woolly, or in somewhat exaggerated refinement in the name may be attributed to the term 'wool,' all sorts of meanings akin to wool; in every case there is wanting to all the Indios the crinkling of the hair from its exit out

of the follicle, whereby would result wide or narrow spiral tubes and the coarse appearance of the so-called 'peppercorn.' The hair of all Indios is smooth and straightened out, and when it forms curves they are only feeble, and they make the whole outward appearance wavy or, at most, curled.

But within this wavy or curled condition of the hair there are again differences. In my former communication I called attention to examinations which I made upon a large number of islands in the Malay Sea, and in which it was shown that a certain area exists which begins with the Moluccas and extends to the Sunda group, in which the hair shows a strong inclination to form wavy locks, indeed passes gradually into crinkled, if not into spiral, rolls. Such hair is found specially in the interior of the islands, where the so-called aboriginal population is purer and where for a long time the name of Alfuros* has been conferred on them. On most points affinity with Negritos or Papuans is not to be recognized. Should such at any time have existed, we are a long way from the period when the direct causes thereof are to be looked for. In this connection the study of the Philippines is rich with instruction. In the limits of the almost insular, isolated Negrito enclave, mixtures between Negritos and Indios very seldom surprise one, and never the transitions that can have arisen in the post-generative time of development. [The island of Negros, on the contrary, is peopled by such crossbreeds.—Translator.]

If there are among the bright-colored islanders of the Indian Ocean Alfuros and Malays close together there is nothing against coming upon this contrast in the Philippine population also. Among the more central peoples the tribal differences are so great that almost every explorer stumbles on the question of mixture. There not only the Dayaks and the other Malays obtrude themselves, but also the Chinese and the Mongolian peoples of Farther India. Indeed, many facts are known, chiefly in the language,† the religion, the domestic arts, the agriculture, the pastoral life which remind one of known conditions peculiarly Indian. The results of the ethnologists are so tangled here that one has to be cautious when one or another of them draws conclusions concerning immigrations, because of certain local or territorial specializations. Of course, when a Brahmanic custom occurs anywhere it is right to conclude that it came here from India. But before assuming that the tribe in which such a custom prevails itself comes from Hither or Farther India, the time has to be ascer-

* On this objectionable name, see supra. That the term does not connote hair characters cf. A. B. Meyer, Sitzungsb. d. Phil. Hist. Classe der kaiserlichen Akademie der Wissensch. Wien, 1882, Vol. CI, p. 550.—Translator.

† Don T. H. Pardo de Tavera, El sanscrito e la lengua tagalog. Paris, 1887.

tained to which the custom is to be traced back. The chronological evidence leads to the confident belief that the custom and the tribe immigrated together.

Over the whole Philippine Archipelago religious customs have changed with the progress of external relations. Christianity has in many places spread its peculiar customs, observances and opinions, and changed entirely the direction of thought. On closer view are to be detected in the midst of Christian activities older survivals, as ingredients of belief which, in spite of that religion, have not vanished. Before Christianity, in many places, Islam flourished, and it is not surprising to witness, as on Mindanao, Christian and Mohammedan beliefs side by side. But, before Islam, ancestor worship, as has long been known, was widely prevalent. In almost every locality, every hut has its Anito with its special place, its own dwelling; there are Anito pictures and images, certain trees and, indeed, certain animals in which some Anito resides. The ancestor worship is as old as history, for the discoverers of the Philippines found it in full bloom, and rightly has Blumentritt* characterized Anito worship as the ground form of Philippine religion. He has also furnished numerous examples of Anito cult surviving in Christian communities.

Chronology has a good groundwork and it will have to observe every footprint of vanishing creeds. Only, it must not be overlooked that the beginning of the chronology of religion has not been reached, and that the origin of the generally diffused ancestor worship, at least on the Philippines, is not known. If it is borne in mind that belief in Anitos is widely diffused in Polynesia and in purely Malay areas, the drawing of certain conclusions therefrom concerning the prehistory of the Philippines is to be despaired of.

Next to religious customs, among wild tribes fashions are most enduring. Little of costume is to be seen, indeed, among them. Therefore, here tattooing asserts its sway. The more it has been studied in late years the more valuable has been the information in deciding the kinship relations of tribes. Unfortunately, in the Philippines the greater part of the early tattoo designs have been lost, and the art itself is also nearly eliminated. But since the journey of Carl Semper† it has been known that not only Malays but also Negritos tattoo; indeed, this admirable explorer has decided that the 'Negroes of the East Coast' practise a different method of tattooing from that of the Marivales in the west, and on that account they attain different results. In the one case a needle is employed to make fine holes in the skin in

* Der Ahnencultus und die religiösen Anschauungen der Malaien des Philippinen-Archipels. Wien, 1882, p. 2. (From Mittheil. der K. K. Geograph. Gesellschaft).

† Die Philippinen und ihre Bewohner. Würzburg, 1869, pp. 50, 137.

which to introduce the color; in the other long gashes are made. In the latter case prominent scars result; in the former a smooth pattern. But these combined patterns are on the whole the same, instead of rectilinear figures. Schadenburg has the operations commence with a sharpened bamboo on children 10 years of age.* Among the wild tribes of the light-colored population tattooing is not less diffused, but the patterns are not alike in the different tribes. Isabelo de los Reyes reports that† the Tinguianes, who inhabit the mountain forests of the northern cordilleras of Luzon, produce figures of stars, snakes, birds, etc., on children 7 to 9 years old. Hans Meyer describes the patterns of the Igorrotes.‡ There appears to exist a great variety of symbols; for example, on the arms, straight and crooked lines crossing one another; on the breast, feather-like patterns. Least frequently he saw the so-called Burik designs, which extended in parallel bands across the breast, the back, and calves, and give to the body the appearance of a sailor's striped jacket. It is very remarkable that the human form never occurs.

What is true concerning tattooing on so many Polynesian islands holds also completely here. But reliable descriptions are so few, and especially there is such a meager number of useful drawings, that it would not repay the trouble to assemble the scattered data. At least it will suffice to discover whether among them there are genuine tribal marks or to investigate concerning the distribution of separate patterns. Those known show conclusively that in the matter of tattooing the Filipinos are not differentiated from the islanders of the Pacific; they form, moreover, an important link in the chain of knowledge which demonstrates the genetic homogeneity of the inhabitants. The tattooings of the eastern islanders are comparable only to those of African aborigines, with which last they furnish many family marks, made out and recognized. It is desirable that a trustworthy collection of all patterns be made before the method becomes more altered or destroyed.

Next to the skin, among the wild tribes the teeth are modified in the most numerous artificial alterations. The preferable custom, common in Africa, of breaking out the front teeth in greater or less number has not, so far as I remember, been described among the Filipinos; I only mention that while I was making a revision of our Philippine crania, two of them turned up in which the middle upper incisors had evidently been broken out for a long time, for the alveolar border had shrunk into a small quite smooth ridge, without a trace of an alveolus. It is otherwise with the pointing of the incisors, especially the upper

* Zeitschrift für Ethnologie, 1880, XII, p. 136.

† Die Tinguianen (Luzon). Translated from the Spanish by F. Blumentritt (Mitth. der K. K. Geograph. Ges. in Wien), 1887.

‡ Verhandl. der Berliner Gesellsch. für Anthropologie, 1883, p. 380.

ones, which, also, is not common. I must leave it undecided whether the sharpening is done by filing or by breaking off pieces from the sides. The latter should be in general far more frequent. In every case the otherwise broad and flat teeth are brought to such sharp points as to project like those of the carnivorous animals. I have met with this condition several times on Negrito skulls and furnished illustrations of them.* On a Zambal skull, excavated by Dr. A. B. Meyer and which I lay before you, the deformation is easy to be seen. I called attention at the time to the fact that among the Malays an entirely different method of modifying the teeth is in vogue, in which a horizontal filing on the front surface is practised and the sharp lower edge is straightened and widened. Already the elder Thévenot has accentuated this contrast when he says:

"These cause the teeth to be equal, those file them to points, giving them the shape of a saw."†

This difference appears to have held on till the present; at least no skull of an Indio is known to me with similar deformation of the teeth. This custom of the Negritos is so much more remarkable since the chipping of the corners of the teeth is widely spread among the African blacks.

The other part of the body used most for deformation—the skull—is in strong contrast to the last-named custom. Deformed crania, especially from older times, are quite numerous in the Philippines; probably they belong exclusively to the Indios. If they exist among the Negritos, I do not know it; the only exception comes from the Tinguianes, of whom J. de los Reyes reports their skulls are flattened behind (por detrás oprimido). Such flattening is found, however, not seldom among tribes who have the practise of binding children on hard cradle boards—chiefly among those families who keep their infants a long time on such contrivances. A sure mark by which to discriminate accidental pressure of this sort from one intentionally produced is not at hand; it may be that in accidental deformation oblique position of the deformed spot is more frequent; at any rate, the difference in the Philippines is a very striking one, since there not so much the occiput as the front and middle portions suffer from the disfigurements, and thereby deformations are produced that have had their most perfect expression among the ancient Peruvians and other American tribes.

I have discussed cranial deformation of the Americans in greater detail, where I exhibit the accidental and the artificial (intentional)

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* Abhandlung über alte und neue Schädel, in F. Jagor's Reisen in den Philippinen. Berlin, 1873, p. 374, Pl. II, figs. 4 and 5.

† G. A. Baer (Verhandl. d. Berliner Anthrop. Gesellschaft, 1879, p. 331) says that such an operation obtains only among Negritos of pure blood.

deformation in their principal forms.* The result is that in large sections of America scarcely any ancient skulls are found having their natural forms, but that the practise of deformation has not been general; moreover, a number of deformation centers may be differentiated which stand in no direct association with one another. The Peruvian center is far removed from that of the northwest coast, and this again from that of the Gulf States. From this it must not be said that each center may have had its own, as it were, autochthonous origin. But the method has not so spread that its course can be followed immediately. Rather is the supposition confirmed that the method is to be traced to some other time, therefore that somewhere there must have been a place of origin for it. On the Eastern Hemisphere, and especially in the region here under consideration, the relations are apparently otherwise. Here exist, so far as known, great areas entirely free from deformation; small ones, on the other hand, full of it. There are here, also, deformation centers, but only a few. Among these, with our present knowledge, the Philippines occupy the first place.

The knowledge of this, indeed, is not of long duration. Public attention was first aroused about thirty years ago concerning skulls from Samar and Luzon, gathered by F. Jagor from ancient caves, to furnish the proof of their deformation. Up to that time next to nothing was known of deformed crania in the oriental island world. First through my publication† the attention of J. G. Riedel, a most observant Dutch resident, was called to the fact that cranial deformation is still practised in the Celebes, and he was so good as to send us a specimen of the compressing apparatus for infants (1874).‡ Compressed crania were also found. But the number was small and the compression of the separate specimens was only slight. In both respects what was observed in the Sunda islands did not differ from the state of the case in the Philippines. Through Jagor's collections different places had become known where deformed crania were buried. Since then the number of localities has multiplied. I shall mention only two, on account of their peculiar position. One is Cagraray, a small island east of Luzon, in the Pacific Ocean, at the entrance of the Bay of Albay;§ the other, the island of Marinduque, in the west, between Luzon and Mindoro. From the last-named island I saw, ten years ago, the first picture of one in a photograph album accidentally

* Crania ethnica Americana. Berlin, 1892, p. 5, and figs.

† Zeitschr. für Ethnologie, 1870, Vol. II, p. 151.

‡ The same, III, p. 110, Pl. V, fig. 1; Verhandl. Anthrop. Ges., Vol. VI, p. 215; Vol. VII, p. 11; Vol. VIII, p. 69; Vol. IX, p. 276.

§ Verhandl. der Berliner Anthrop. Gesellsch., 1879, Vol. XI, p. 422; 1889, Vol. XXI, p. 49.

placed in my hands. Since then I had opportunity to examine the Schadenberg collection of crania, lately come into the possession of the Reichsmuseum, in Leyden, and to my great delight discovered in it a series of skulls which are compressed in exactly the same fashion as those of Lanang. It is said that these will soon be described in a publication.

It is of especial interest that this method has been noted in the Philippines for more than three hundred years. In my first publication I cited a passage in Thévenot where he says, on the testimony of a priest, that the natives on some islands had the custom of compressing the head of a newborn child between two boards, so that it would be no longer round, but lengthened out; also they flattened the forehead, which they looked upon as a special mark of beauty. This is, therefore, an ancient example. It is confirmed by the circumstance that these crania are found especially in caves, from the roofs of which mineral waters have dripped, which have overlaid the bones partly with a thick layer of calcareous matter. The bones themselves have an uncommonly thick, almost ivory, fossil-like appearance. Only the outer surface is in places corroded, and on these places saturated with a greenish infiltration. It is to be assumed, therefore, that they are very old. I have the impression that they must have been placed here before the discovery of the islands and the introduction of Christianity. Their peculiar appearance, especially their angular form and the thickness of the bone, reminds one of crania from other parts of the South Sea, especially those from Chatham and Sandwich Islands. I shall not here go further into this question, but merely mention that I came to the conclusion that these people must be looked upon as proto-Malayan.

The changes which will take place in the political condition of the Philippines may be of little service to scientific exploration at first; but the study of the population will be surely taken up with renewed energy. Already in America scholars have begun to occupy themselves therewith. A brief article by Dr. Brinton is to be mentioned as the first sign of this.* But should the ardent desire of the Filipinos be realized, that their islands should have political autonomy, it is to be hoped that, out of the patriotic enthusiasm of the population and the scientific spirit of many of their best men, new sources of information will be opened for the history and the development of oriental peoples. To this end it may be here mentioned, by the way, that the connecting links of ancient Philippine history and the customs of these islands, as well with the Melanesians as with the Polynesians of the south, are yet to be discovered.

* The Peoples of the Philippines, Washington, D. C., 1898. American Anthropologist.

As representatives of these two groups, I present, in closing, two especially well-formed crania from the Philippines. One of them, which shows the marks of antiquity that I have set forth, belongs to an Indio. It has the high cranial capacity of 1,540 cubic centimeters, a horizontal circumference of 525 millimeters, and a sagittal circumference of 386 millimeters; its form is hypsidolicho, quite on the border of mesocephaly: Index of width, 75.3; index of height, 76.3. Besides, it has the appearance of a race capable of development; only, the nose is platyrrhine (index, 52.3), as among so many Malay tribes, and in the left temple it bears a *Processus frontalis squamae temporalis* developed partly from an enlarged fontanelle. The other skull was taken from a Negrito grave of Zambales by Dr. A. B. Meyer. It makes, at first glance, just as favorable an impression, but its capacity is only 1,182 cubic centimeters; therefore 358 cubic centimeters less than the other. Its form is orthobrachycephalic; breadth index, 80.2; height index, 70.6. As in single traits of development, so in the measurements, the difference and the debased character of this race obtrude themselves. Only, the nasal index is somewhat smaller; on the whole, the nose has in its separate parts a decidedly pithecoid form.

SCIENCE AND PHILOSOPHY.

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WHETHER the average man recognize the situation or palter with it, there can be no doubt that a dualism, a separation, if not an antagonism, between science and religion forms one conspicuous phenomenon of modern life. True, palliating circumstances may have eased or disguised it somewhat in recent years. But palliation was always a makeshift, and, everything considered, the fundamental opposition remains, little mitigated. Of course, we may allege that religion and theology are by no means identical, and that the latter rather than the former withstands scientific views and conclusions. Yet, when we summon courage to be quite frank with ourselves, we must admit freely that religion, as an organized social factor, is so bound up with theological presuppositions as to render this distinction more of a subterfuge than a solution. Again, many in these days seem to think that another marked contrariety of interest characterizes the relation between science and philosophy. And, indeed, one must admit that, altogether apart from theoretical problems, certain features of the academic world, in such countries as Scotland and the United States, for example, afford basis for this prevalent opinion. In the Scottish universities one-half of the professors of philosophy are clergymen. In the universities and colleges of the United States the theological affiliations are even more intimate. Speaking from memory, I recall that in only three of our nine leading universities do we find the philosophical departments free from clerical influence, while, in the lesser institutions, clerical control constitutes the rule, not the exception. Small wonder, then, if many have identified the tendencies of philosophy with the theological, as opposed to the scientific, side of contemporary controversy.

But, if manifold causes thus support the view that science and philosophy must necessarily conflict, there happen to be other aspects of the matter well worth consideration. In Germany, for instance, the home land of philosophical inquiry during the nineteenth century, the progress of science has been exercising decisive influence on speculative thought for a generation at least. Furthermore, the rise and development of experimental psychology has induced many, whose main work lies on the philosophical rather than on the scientific side

of the fence, to familiarize themselves with the scientific attitude and temper, while the gradual entry of the sciences into the usual undergraduate course at our colleges has not left the earlier education of those who come afterwards to specialize in philosophy so utterly void of scientific knowledge as it used to be. Moreover, the long commerce of our ablest students with German culture cannot but have produced similar modifications. Be this as it may, the point I desire to emphasize is that no conflict can exist properly between science and philosophy, and that a most interesting—possibly the most hopeful—trait of recent thought may be traced precisely in the inclination towards an alliance. It ought to be said, too, that some few, whose work appears to lie in the immediate future—to present symptoms of decided vitality—tend clearly in this direction. And, when we stay to reflect for a moment, why should it not be so? Science and philosophy possess this in common—they search for the truth free from all trammels of dogmatic presupposition. If they prove true to themselves, their object must be the same, even if they view it from different sides and for different purposes. Take them from what standpoint you please, both are 'science' in the broad sense of the untranslatable term, *Wissenschaft*. It may be of interest, therefore, to devote some attention to the new-old question, What is the relation between science and philosophy?

Approaching the problem historically, it proves something of a shock to learn that the so-called opposition had no existence seventy years ago. Nay, from the time of Descartes' 'Discours de la Méthode' (1637) till the enunciation of the cellular theory (Schleiden and Schwann, *c.* 1838), free interaction, often conscious cooperation, prevailed. Recall the full title of Descartes' epoch-making tractate, 'Discours de la Méthode pour bien conduires sa raison, et chercher la vérité dans les sciences; plus la Dioptrique, les Météores et la Géométrie, qui sont des Essais de cette Méthode'; recall Spinoza, the optician; recall Leibnitz and his calculus; recall the sober, scientific temper of the entire British school, from Hobbes to Hume; recall Kant's cosmogony, the precursor of modern ethereal physics, and remember that the critical philosopher likened himself, not to Plato or Bacon, but to Copernicus; you find no ground for controversy, but every symptom of mutual good-will. The contrast between this two hundred years' truce, covering the history of thought from the Reformation till the French Revolution, and the undignified, profitless squabbles, still fresh in the memory of many middle-aged men, is so striking that a call for reasons goes forth at once. If this matter can be elucidated, much will have been done to *explain away* the recent unfriendliness. At the same time, the case presents peculiar difficulties, because the evolution of thought in this connection furnishes

one among the great paradoxes of history. Ever fond of revenges, the Time-spirit becomes supremely ironical here.

Post-Reformation Europe accomplished much for science and philosophy. In both fields, investigation followed clearly marked lines, but the conclusions reached were of such a nature that they dovetailed easily. For science meant the mathematico-physical sciences, specifically, mathematics, astronomy and 'molar' physics. Philosophy meant, on the continent, the metaphysics of dualism, starting from the question, How can matter and mind, extension and thought, as they were then termed, be related so as to form a consistent whole; in Britain, individualistic psychology, concentrated on the problem, How do I get knowledge, and, when obtained, what is it? In a word, the sciences and philosophy attacked the same universe—the universe as conceived by Newton. Philosophy did not aspire to a higher knowledge than that reached by science, but confined its inquiries to some aspects of the world which had been left untouched by mathematics and physics. Thus both arrived at consonant conclusions. Harvey, for example, did not suggest that Bacon wrote on scientific questions like a philosopher, as he would assuredly have done had they lived forty years ago; he said merely, the Lord Chancellor writes like a Lord Chancellor—a lawyer of assured position.

The general view of the universe then held by scientific men supplied the framework within which the philosophers labored; it did not occur to the metaphysicians that the modes of thought in which this universe was conceived could be subjected to fundamental criticism. What, next, was this view? Briefly, it may be called static, molar and mechanical in the strictest sense of the word mechanism. It dealt with *self-contained* bodies in equilibrium or at rest; with *self-contained* aggregations of matter capable of measurement; with the relations subsisting between *self-contained* wholes; that is, with external connection, not with internal self-manifestation. As time passed, this general conception of things became more and more firmly rooted, thanks to Newton's genius. Indeed, it maintained itself with little change, especially in the English-speaking countries and in France, till forty-five years of the nineteenth century had winged their way. Whewell, in his great 'History of the Inductive Sciences' (1837-57), displays astonishing ignorance of the transformations that were afoot in his day—of Gauss and Weber on absolute measurements, of Schwann on the cell theory, of Mohl on protoplasm, of Mayer on heat, of Helmholtz on the conservation of energy, of Herapath on the mechanical theory of gases and the like. If the hold of the 'Newtonian philosophy' remained so strong at this date, we can infer readily how exclusive was its predominance in previous times. Now the theory of the universe contained in the *Principia* had a distinctively philosophical aspect

in so far as it was universal. Otherwise it presents few qualities to which we should attach this name at present. For the system contemplated the division of matter into separate parts, each of which occupied a place—a place subject to change, no doubt—in empty space, while to these circling orbs force was linked somehow. The relation between any two, therefore, can not be the result of inherent nature, but must follow from the interference of a cause external to the terms of the relation. Newton has put himself on decided record on this very question. In a letter to Bentley, written about the new year of 1693, he says: "It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon and effect other matter without mutual contact, as it must be, if gravitation, in the sense of Epicurus, be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a *vacuum,* without the mediation of anything else, by and through which their action and force may be conveyed from one to the other, is to me so great an absurdity that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers." Summarily put, this means that the solar system is the type of universe, and it is a *system,* because 'an agent acting constantly according to certain laws' has rendered it such. And this implies, further, that, while a *description* of the universe may be possible, as in terms of mathematics, an *explanation* of it is beyond reach.

Turning to the philosophical side, a curious parallelism attracts notice at once. Of course, we are dealing no longer with 'heavenly bodies,' but matter and mind are treated by the philosophers exactly as Newton dealt with his material wholes. Descartes asks himself in effect, How is it that thought, which possesses none of the qualities of extension (matter) and extension, which possesses none of the qualities of thought, comes to unite so as to *be* a single whole in man's experience? He supposes that ideas are copies of things. But, even so, How do we know that the copies are correct or reasonably adequate? The solution can come by one answer alone. Some agent, which is neither an idea nor a thing, must vouch for the correspondence. Just as, in the physical world, one body can not affect another, except by the operation of a law-giving agent, so thought and extension can not be combined in our universe, except God have so willed it. The parallelism is precise. A 'third thing,' belonging neither to the sphere of intellect nor to that of the external world, must be assumed as the

basis of both. Here we meet that hoary sinner, the 'uncaused cause,' about which, to our modern amazement, the science and philosophy of that day are agreed entirely.

Passing to England, we find Locke confronted by a different problem, but the setting remains identical. Assuming, like Newton and Descartes, two separate factors—others assumed many, but the number makes no essential difference—and asking, How do I, who am inside, get my knowledge of things, which are outside? Under the circumstances, the obvious reply is, through the senses. The senses write upon the mind. Unfortunately, this information about the external world lacks directness, for the senses are modifications of the bodily organism and therefore tell nothing about the real objects. How, then, placed in such a dilemma, do we know objects? Locke alleges that Substance, a thing which we do not perceive, but which we are compelled to infer, originates the conviction of permanence associated with reality in objects. Here, once more, a third thing, belonging to neither of the factors under review, plays the part of Newton's agent and of the Cartesian deity. Without condescending upon further details, it is easy to see why science and philosophy could not well fall out during the period when such conceptions held sway. But this agreement, happy in its unconsciousness of problems at all events, was not to endure forever. The world of human experience revealed new aspects, and fresh questions, sources of dire controversy, loomed upon the horizon. The dynamic, molecular and organic modes of thought, with their attendant conception of the universe, were destined to elbow out the static and mechanical.

Even amid many seeming transformations, the 'Newtonian philosophy' preserved itself unchanged in essentials. The Deistic movement, Butler's 'Analogy,' Pope's 'Essay on Man' and Paley's 'Natural Theology,' and the highly wrought productions of the great French physicists, culminating in Laplace's 'Mécanique Céleste,' even the Scottish 'common-sense' protest against current scepticism, all emerged on the basis of its first principles. But, after the middle of the eighteenth century, three men shook it to its foundations and made possible the new structure we now call 'modern' thought. These men were Hume, Kant and Herder; the half-conscious protest of Spinoza had passed over the heads of his contemporaries unheeded. Was he not a Jew, a pantheist and, therefore, a flat blasphemer? The joint performance of this eighteenth century trinity, 'equal in power and glory,' raises problems of the most complicated kind, so complicated, indeed, that they have been the bugbear even of expert students during the last two generations. I can attempt here to put the salient points only, as clearly as possible.

Many pious efforts to understand Hume have been frustrated by

the idea that he was a dangerous sceptic, an infidel, a bold, bad man and what not. The simple truth happens to be that Hume found himself confronted by certain definite questions which had grown under the hands of his predecessors. In his case, the power of the man coincided with the power of the moment; and he still occupies his lonely pedestal as the single thinker of the first-class produced by the Anglo-Saxon race, because he settled the account of an age once for all. Had this been comprehended sooner, the nineteenth century would have been saved a wealth of waste paper and some lost temper. Reduced to its simplest elements, Hume's central problem is by no means hard to grasp, particularly if the Descartes-Newton scheme be recalled. Granted that *separation* of individuals, whether of men, of material bodies or of thought and extension, constitutes the fundamental fact in the universe; granted, too, that knowledge flows into consciousness through the senses, then what value can be attached legitimately to human experience? Hume, as one must always remember, possessed the wit not to rest satisfied with the dogmas that appeased his forefathers after the intellect. He wanted to know what precise inferences could be extracted from their cherished opinions, and he suspected that their satisfaction had not been won fairly. Accordingly, he showed, and the proof holds good beyond peradventure, that, on this traditional basis, human knowledge can be viewed only as a huge delusion. Objects, self and deity; matter, mind and cause; science and philosophy engulf themselves. Another alternative is impracticable, if the presuppositions, common to the mathematico-physical sciences, to the Cartesian metaphysics and to the British psychology, be admitted. It was no part of Hume's task to examine this foundation. He accepted it without change as it came to him and proved, in the most thoroughgoing fashion, that universal nescience was its sole logical end. Dualism, self-contained bodies, sensationalism, 'an agent acting constantly according to certain laws'—in short, the entire paraphernalia held conjointly by the science and philosophy of the seventeenth and eighteenth centuries, he hoisted with its own petard and blew to shivers irretrievably. For, admit his premises and the conclusion follows resistlessly. Now, the view of the universe prevalent from Descartes to Paley, from Galileo to Laplace, depended on Hume's premises and upon nothing else! So ended the first lesson, like its kind, not to be taken to heart for many a long day.

If the real implications of Hume's argument remained hidden from science, thanks to the continued predominance of the 'Newtonian philosophy,' and from the men who spoke Hume's tongue, thanks to contemporary political and theological causes, the same cannot be said of Kant. His philosophy took Europe by storm and has continued to influence scientific men perhaps more than any other body of philo-

sophical thought. His contribution to our present subject of inquiry consisted of two parts: (*a*) the distinction between *Verstand* and *Vernunft*, (*b*) the conclusions gained in the last division of his masterpiece, the 'Critique of Pure Reason.' The former was destined to exercise decisive effect on the relation between science and philosophy; the latter, as I understand the 'Dialectic,' met much the same fate as Hume's destructive analysis—it was misinterpreted or overlooked. We may therefore take it first, and very briefly. In the third part of the 'Critique of Pure Reason,' entitled 'Dialectic,' because it deals with subjects capable of dialectical treatment, Kant shows that the metaphysic of his predecessors must be adjudged a complete failure. Mathematics and physics exist, for they have objects; but metaphysics has no existence, its objects are unthinkable, humanly speaking. Take the soul (or self) as a *self-contained* thing, occupying a place among the other *self-contained* elements of human life; interpret the universe as a *self-contained* object, one among other objects of experience; conceive God as a *self-contained* 'agent acting constantly according to certain laws,' and residing far out in the depths of space; in a word, let your fundamental conceptions be those of the Descartes-Newton type; then, when you come to analyze them, you will find of a surety that no such soul or universe or God can possibly enter into human experience. This Kant proves, and so cuts the throat of the metaphysic which ruled science and philosophy from the Reformation till his day. On the whole, philosophers have not yet fathomed his meaning, while scientific men have been quick to seize his point, that metaphysic does not exist, forgetting completely that his work was preliminary to the necessary question: What, then, *are* soul, the universe and God? To declare, with a certain quasi-scientific school, that these are mere ideas, helps us not a whit. For the declaration, as they do not see, destroys the validity of science also. Thus, on a broad view, we have still to reckon with this aspect of Kant's thought.

The distinction between *Verstand* (understanding) and *Vernunft* (reason)—the English words fail to translate, unfortunately—stands in very different case, having been productive of momentous consequences. Kant's early scientific researches led him to see that a dynamical account of the material universe ought to be substituted for the static conception of Newton. Indeed, he hit upon the idea of preorganic evolution; but, as thermodynamics lay in the future, experimental evidence lacked, and he was switched on to another line by Hume. According to Hume, knowledge is phenomenal, and phenomenal only. It consists of what apppears to be; can have no commerce with what is. By analysis, the most complex ideas can be proved to possess a phenomenal basis. The faculty of analysis, which deals thus with phenomena, Kant called *Verstand*. But he insisted that Hume's

assumption, that knowledge comes from the senses, did not suffice to explain experience. Man's mind is endowed with certain forms or principles of synthesis, by means of which the sense-material is organized into knowledge. *Vernunft* is the faculty whereby such principles may be apprehended. It implies a higher range and a deeper insight than *Verstand.* This superior faculty, in combination with an amplified reading of Herder's theory of historical evolution, was to be responsible for much, as we shall see in the sequel.

Herder, a younger contemporary of Kant, turned away from the mathematico-physical sciences, to which nearly all great intellects had been attracted for two centuries, and entered enthusiastically upon the study of the history of culture, of culture in the spacious sense of civilization. Even in this line of research, he can not be called an exact student. But his was a vitalizing personality, and so, his limitations notwithstanding, he originated the evolutionary and organic idea which may be termed appropriately *the* nineteenth century standpoint. He took particular delight in poetry, religion, language and the like. As early as 1767, he enunciated the conception which was to create historical science. "There is the same law of change in all mankind and in every individual, nation and tribe. From the bad to the good, from the good to the better and best, from the best to the less good, from the less good to the bad—this is the circle of all things. So it is with art and science; they grow, blossom, ripen and decay. So it is with language also." In the realm of the human spirit, all things work together; "history leads us into the council of fate, teaches us the eternal laws of human nature and assigns us our own place in that great organism in which reason and goodness . . . must create order."

At this point we strike the psychological moment when the conditions that led to the conflict between science and philosophy were assembling. Evidently, the center of gravity of philosophical inquiry would be shifted from the old mathematico-physical parallelism, if a professed philosopher were to appear equipped with the insight and speculative daring requisite to unite Kant's conception of *Vernunft* with Herder's fruitful suggestion, that history is a vast organism 'in which *reason* must create order.' This epoch-making thinker did arise, in the person of Hegel. We can not stay to outline the Hegelian system, but must rest content to state its germinal idea. Following upon Herder's pregnant thought, Hegel conceived of the universe as a single unity, inspired and controlled by a principle of reason, a principle in and through and for which everything has being. Obviously, if the human mind can grasp such a principle, Kant's faculty of *Vernunft* is the one power endowed with the necessary ability. As obviously, on these conditions, if a thinker can pick out, as it were, the rational

forms under which this principle manifests itself, he will have mastered the mystery of all things. From the year 1818, the date of Hegel's election to the chair of philosophy in the University of Berlin, till the break-up of his school, about 1850, his thought dominated the intellect of Germany to a degree unparalleled, and from 1865 till the present time, it has wielded power in the British and, to a lesser extent, in the American universities. The reason for this is patent. No other thinker entertained *modern* views. In the English-speaking countries particularly, men faced the past, not the future. Hegel, on the contrary, whatever may be said in his despite, had carried the dynamic, organic and evolutionary explanation into every corner of the humanistic realm. Nevertheless, he and his disciples must bear the chief responsibility for the estrangement between science and philosophy throughout forty years (1850-90) of the nineteenth century. Why?

In the first place, this, the most influential system of modern philosophy, had been completed to all intents and purposes by the year 1816. And, unfortunately, this statement implies another. In 1816, modern science was as yet unborn. Of course, one does not forget the work of Haller, at Göttingen; of Cuvier and Bichat; of Treviranus, who was the first to use the term 'biology,' in 1802; and, above all, one calls to mind Charles Bell's capital discovery, in 1811. Still, all these died in the faith, they having received not the promises. In France, mathematical science maintained its glorious history, thanks partly to the favor of Napoleon. In Germany, the rule of the modern scientific spirit dates from 1826, with the foundation of Liebig's laboratory at Giessen. In Britain, all the great advances, Bell's excepted, fall within the domain of astronomy, physics and the older chemistry. Yet, despite this meager knowledge, as we deem it now, of the intricacies of nature, a thinker dared to present an absolute philosophy—a key to all the mysteries.

In the second place, the interpretation of Hegel's system by his followers, if not its elaboration by himself, had become increasingly formal, perhaps abstract, just at the moment when science was making some of its most astonishing discoveries. Small wonder, then, that investigators of nature, successful beyond all precedent, turned in contempt from a philosophy which seemed to them, rightly or wrongly, a species of revived scholasticism. Moreover, the bitter attacks on Hegel emanating from workers on the philosophical side, like Herbart and his pupils, who appeared to be, and possibly were, sympathetic with scientific methods, served to deepen this impression. By 1865, when the cry, 'Back to Kant!' had taken effective possession of many and was emphasizing the importance, for science, of a certain interpretation of Kant's thought, this antagonism crystallized finally.

Lastly, Hegel's 'Naturphilosophie,' containing his account of those

phenomena of nature to which he attempts to apply his principles, was the weakest spot in his system. As the sciences progressed, this became more and more evident, and it would have been asking too much of human nature to have required the enemy to forego this grand opportunity for telling assault. No doubt, these attacks went too far, as the formative influence of the 'Philosophy of Nature' upon men like Oken, Oersted, K. E. von Baer, Johannes Müller and Schönlein proves overwhelmingly. Nevertheless, the fact remains that the insights of 'Naturphilosophie' were not restored to scientific citizenship till late in the century, and its unchastened speculations alone attracted general attention in pre-Darwinian times. In this field, the decisive one for science, be it remembered, the vaunted higher and special knowledge of philosophy was adjudged guilty of ludicrous error, of gross carelessness, of otiose imaginings.

While the newer science thus scouted the new philosophy, was it without sin? In answering this question, we come upon what I have called one of the greatest paradoxes of history. Just after the nineteenth century had passed its zenith, a group of writers, penetrated by the dynamical and biological tendencies of contemporary science, thought that the times were ripe for an accordant theory of the universe. The discoveries of Wöhler, who produced an organic substance by the synthesis of inorganic materials; the startling advances of biology, especially in the physiological line; and the speculations of such thinkers as Schopenhauer and Feuerbach, appeared to furnish a ground for *scientific* explanation of certain factors in experience which had defied interpretation hitherto. Thus, despite its contempt for the regnant philosophy, science, stimulated by its own problems, produced a philosophical theory. Opponents of this movement, like opponents in all ages, thought to get rid of an irritating novelty by means of a nickname. Accordingly, we hear of the *monism* of Moleschott, Büchner, Carl Vogt and Haeckel. By applying this title, critics intended to indicate that these thinkers suppressed the great differences of experience—the difference between matter and mind or between the organic and the inorganic—and saddled one term, in this case, matter or the inorganic, with the entire responsibility of a solution. Now, it is true that this school alleged matter to be the cause of mind, that they said, 'brain secretes thought as the liver secretes bile,' and did many other things equally foolish or objectionable. At the same time, their critics stood too near them and a clearly defined focus was unobtainable. The real fact was—and here the paradox emerges—that Büchner and the rest set up, not a monism, but a dogmatism. Despite the circumstance that the progress of science, to say nothing of Hume and Kant, had demonstrated beyond doubt the insufficiency and fallacy of the dualistic, static and analytic theory of

the universe, they *accepted this as a presupposition.* To begin with, matter and mind were not one, but two; they were different, that is. Given this difference, then, how explain them? By showing that, in the time series, mind came second, and was therefore caused by matter. The laughable paradox is that men steeped in the biological view, which utterly overturns this mechanical externality, adopted the latter as *the* means adequate to account for the former! Of course, a man may do this, if he please, but at his peril. For, Hume and Kant and the biological sciences have combined to show that, even before it could be stated, this doctrine had become, not merely untenable, but positively unthinkable. It was now the philosophers' turn to blaspheme their brethren of science. If the errors of 'Naturphilosophie' had handed speculative thought over to the tender mercies of exact science, the ludicrous obtuseness of the so-called materialists respecting what was possible in philosophy brought the thinkers their due revenge. Thus the dispute became interminable and the Jew had no dealings with the Samaritan. For science, philosophy appeared so much vague or formal speculation; for philosophy, science, in so far as it tried to explain the world, seemed nothing but a blind blundering among exploded errors peculiar to Locke and the French encyclopedists. Despite Lotze's effort at mediation, too complacent towards both parties to command the respect of either, this was the substantial situation from 1850 till 1885. And, when we hear to-day of the opposition between science and philosophy, our ears are really ringing with echoes from the period of the great paradox. The later developments of physics, chemistry, biology and psychology have brought scientific men to a point where they can see that the mere adoption of the 'Newtonian philosophy,' minus the 'agent acting constantly according to certain laws,' is a far too simple solution of the obscure problems on hand. Seductive it may be, it fails notoriously to fill the bill. Similarly, philosophers begin to understand that Hegelianism must go as a system, even though they feel that they must retain Hegel's one contribution to progress—the principle that experience can be explained, if at all, only by reference to itself. Also they evince symptoms of perceiving that the watchword, 'Back to Kant!' valuable enough in 1860, must be replaced by the new rallying cry, 'Forward from Kant.' The critical philosophy cleared a site upon which it is possible and proper for science and speculation to cooperate in building now.

Science and philosophy may easily return to the old footing, then, if they will but have a mind to rid themselves of the peculiar dogmas that have afflicted each during the last century. This implies mutual self-sacrifice, but sacrifice of the unimportant, very likely of the harmful. There is no good ground for the belief that with the circle

of the positive sciences knowledge ends; for the naïve supposition that an object can exist without a subject; for the marvelous delusion that observation and experiment are capable of revealing things new and old without the aid of mental synthesis or of psychological volition; for the charming inconsequence that we perceive phenomena and are therefore ignorant of reality. But equally, no basis can be found for the idea that philosophy has means of access to some special knowledge denied science; that one can afford to neglect science in favor of rational forms; that the conclusions of physics, chemistry and biology are subject to revision at a higher tribunal; or that the work of the sciences is a monstrous delusion. On the contrary, there is every reason for insisting that science and philosophy are interwined inextricably—much more inextricably now than they could have been in Newton's time. Both work upon the same *closed* universe. This is the important fact, even if science inquires, What is it? philosophy, What does it mean? Nay, both questions are unanswerable, and so the two disciplines alike end in approximation and hypothesis. As Romanes has put it, "The 'Origin of Species' first clearly revealed to naturalists as a class that it was the duty of their science to take as its motto what is really the motto of natural science in general, 'Felix qui potuit rerum cognoscere causas,' not facts, then, or phenomena, but causes or principles are the ultimate objects of scientific quest." They are the objects of philosophical quest also, as Romanes shows elsewhere. In a word, to become conscious of its own fundamental principles, science must transform itself into a kind of philosophy, while to become acquainted with its own illustrative material, philosophy must transform itself into a kind of science. This way lie harmony and progress. We expect the twentieth century to furnish forth the imperative eirenicon. It can not come too soon.

A STUDY OF BRITISH GENIUS.

BY HAVELOCK ELLIS.

IX.—PERSONAL CHARACTERISTICS.

BEFORE summarizing the results of this study and noting a few of the conclusions to which it seems to point, there are still some aspects of British men of genius that the 'Dictionary' serves to make visible. And as these aspects enable us at once both to complete our picture and to confirm some of the impressions we have already obtained, we cannot afford to pass them by. They concern more especially personal appearance and emotional disposition.

As regards stature we have some information in 281 cases; in 218 cases the information is indefinite, in the remaining 63 cases definite. Of the first and largest group, 91 are said to be tall, 53 of average or medium height, while 74 are short. In the smaller group, composed entirely of males, 4 are 5 feet and under;* 5 are from 5 feet 1 to 5 feet 4; 14 are from 5 feet 5 to 5 feet 8; 26 are from 5 feet 9 to 6 feet; 14 are over 6 feet. The height of the average Englishman at the present day is 5 feet 8. It may be added that among the general population of the British Islands 68 per cent. are between 5 feet 4 inches and 5 feet 9 inches in height. But the average height of men of the well-to-do classes, to which our subjects mainly belong, is somewhat above this. If we say that it is 5 feet 9, we shall probably be near the mark. This is confirmed by Galton, who found that the average height of the fathers of his men of science was 5 feet 9¼. But if this is so, it would appear that it is the tendency of our men of genius not only to vary widely, but to be tall more frequently than short.† The center of the group is really occupied by the individuals who are 5 feet 10, since 29 are below this height and 27 above it.

It must, of course, be recognized that various fallacies would be involved were we to take our data as strictly corresponding to the real facts. The exceptional people are more likely to be mentioned, and the medium-sized to be passed over, while there is always a tendency to describe a person as short or tall, rather than as of average size. It

* Pope, 4 feet 6, is excluded, as his excessively low stature was the result of deformity.

† I may remark that among the ordinary population there is some reason to suppose that superior intellectual capacity tends to be associated with superior stature; Porter found such an association among school children at St. Louis and Christopher at Chicago.

may be noted, however, that the group for which we have definite figures harmonizes fairly with the group for which we have no definite figures, and that both alike show that the number of medium-sized persons is vastly below what we ought to expect. Moreover, the group with definitely ascertained heights shows very wide range of variation. When we note that among some 850 men there are 14 who are definitely known to have been over 6 feet in height, and many others who are known to have been 'gigantic' or 'colossal,' we may be fairly certain that more definite knowledge would only show more clearly that the relations that rule here are not exactly the same as those that rule among the general population, and that men of intellectual ability show in this respect a greater tendency to variation than is observed among the general population.*

It is interesting to note that although among the general population the well-to-do classes are decidedly taller than the lower social classes, no such tendency is clearly marked in our groups. Confining ourselves to the group with definitely known height, we find that none belong to our 'good family' class, while two belong to our lowest social class, springing from unskilled workers. The extremely small persons belong to the middle or lower middle social classes. This seems to indicate that height is here not a mere social phenomenon, but a real expression of the organic vitality and nervous make of the man.

It would be of much interest if we could speak definitely concerning the most important of all anthropological criteria, the cephalic index or length-breadth index of the head. The 'Dictionary' here, however, is of no assistance. We are told, indeed, of Faraday (the writer of the article being Tyndall) that he had an abnormally long head, so that his hats had to be specially made for him, and we are told of Tyndall himself (the writer here being his widow) that in this respect Tyndall resembled Faraday. This scrap of evidence, so far as it goes, would confirm the proverbial belief in favor of the intelligence of long-headed persons. It is, however, believed by many, who can bring forward good evidence on their side, that intellectual ability goes with broad-headedness. It may well be that in this matter, as in that of stature, the range of variation is great, and that both extremes tend to prevail to an undue extent. This has been found to be the case in another abnormal group—that of criminals.

If we turn to a further anthropological character, pigmentation, or the color of the hair and eyes, we are able to bring forward a much larger body of evidence, and it is not difficult to supplement the data

* This conclusion harmonizes with an inquiry into this matter, and into its significance—not, however, confined to persons of British race—which I published elsewhere a few years ago ('Genius and Stature,' 'Nineteenth Century,' July, 1897).

furnished by the 'Dictionary' by the help of portraits, more especially those in the National Portrait Gallery. I have information on this point concerning 334 of the eminent persons on our list. In classifying by pigmentation I have relied in the first place on the eye-color, but have allowed hair-color a certain influence in modifying the class in those cases in which there was marked divergence between the two in lightness or darkness. I have sorted the eminent persons into three classes, according as their eyes were unpigmented (blue), highly pigmented (brown), or occupying an intermediate position (combinations of blue with yellow, orange or brown). This intermediate class has necessarily been large, and I have comprised within it three subdivisions: a fair medium, a dark medium, and, between these two, a doubtful medium. Then the question arose as to how the results thus obtained might be conveniently formulated, so as to enable us to compare the different groups of eminent persons. I finally decided to proceed with each of these groups as follows: The doubtful medium persons in each of these classes were divided equally between the fair medium and the dark medium; then two-thirds of the fair-medium persons were added to the fair class, the remaining third to the dark class, and, likewise, two-thirds of the dark medium were added to the dark class, the remaining third to the fair class; the five classes were thus reduced to two, and, on multiplying the fair by 100 and dividing by the dark, we obtain what may be called an index of pigmentation. This method of notation is really simple, and is quite sufficiently accurate for the nature of the data dealt with; it will be seen that by its use an index of 100 means that fair and dark people are equally numerous in a group, while indices over 100 mean an excess of fair persons, and indices under 100 an excess of dark persons. I have been able to obtain the index of pigmentation in the cases of ten groups, the remaining groups being too small to permit of assured results. I present them, with their index of pigmentation, in the order of decreasing fairness: Men of science, 150; Artists, 108; Lawyers, 100; Sailors, 100; Soldiers, 83; Statesmen, 83; Poets, 78; Men of letters, 67; Divines, 43; Actors, 20.

It will be seen that the range is considerable; but I believe we may have considerable confidence in the results, and the more so since they are not altogether unexpected, for (although I do not wish to assume that these phenomena are entirely explicable by race) it is certainly true that men of science and artists tend largely to come from fair districts of the country, divines and actors from dark districts. The fact that allied classes tend to fall together—soldiers and sailors, poets and men of letters—also gives confidence in the reality of the relationship thus brought out. It may be noted, as a fact probably not without significance, that the more active and unemotional classes tend

to be fair, while the more reflective and emotional classes tend to be dark.*

There is another physical characteristic to which the national biographers frequently allude, though I do not propose to attempt to give it any numerical values, and that is personal beauty or the absence of it. A very large proportion of persons are referred to as notably handsome, comely, imposing; a very considerable, but smaller, proportion are spoken of as showing some disproportion or asymmetry of feature, body or limbs, as notably peculiar or even ludicrous in appearance. A not uncommon type is that of the stunted giant, with massive head and robust body, but very short legs.

There is one feature, however, which is noted as striking and beautiful in a very large number of cases, even in persons who are otherwise wholly without physical attractions. That is the eyes. It is nearly always found that descriptions of the personal appearance of men of genius, however widely they may differ in other respects, agree in finding an unusual brilliancy of the eyes. Thus the eyes of Burns were said by one observer to be like 'coals of living fire,' and Scott writes that they 'literally glowed'; while of Chatterton's eyes it was said that there was 'fire rolling at the bottom of them.' It is significant that both of these instances, chosen almost at random, were poets. While, however, the phenomenon seems to be noted more frequently and with more emphasis in poets, it is found among men of genius of all classes. One can only suppose it to be connected with an unusual degree of activity of the cerebral circulation.

In regard to the mental and emotional disposition of British persons of genius, the national biographers enable us to trace the prevalence of one or two tendencies. One of these is shyness, bashfulness or timidity. This is noted in thirty-four cases, while thirty-two others are described as very sensitive, nervous or emotional, and, although this is not equivalent to a large percentage, it must of course be remembered that the real number of such cases is certainly very much larger, and also that the characteristic is in many cases extremely well marked. Some had to abandon the profession they had chosen on account of their nervous shyness at appearing in public; others were too bashful to declare their love to the women they were attracted to; Sir Thomas

*I may remark concerning this index of pigmentation that, while it yields results which are strictly comparable among themselves in the hands of a single observer, proceeding in a uniform manner, it is doubtful whether two observers would carry it out in a strictly identical manner. Beddoe's index of nigrescence, founded on hair-color and applied directly to living subjects, is a convenient formula for indicating the degree of pigmentation. But in my observations, largely made on portraits (in which the hair was often whitened by age, absent, concealed beneath a wig, or obscured by the darkening of the paint), it was necessary to accept eye-color as the primary basis of classification.

Browne, one of the greatest masters of English prose, was so modest that he was always blushing causelessly; Hooker, one of the chief luminaries of the English Church, could never look any one in the face; Dryden, the recognized prince of the literary men of his time, was, said Congreve, the most easily put out of countenance of any man he had ever met. It is not difficult to see why the timid temperament —which is very far from involving lack of courage*—should be especially associated with intellectual aptitudes. It causes a distaste for social contact and so favors those forms of activity which may be exerted in solitude, these latter, again, reacting to produce increased awkwardness in social relations. Moreover, the mental state of timidity, which may be regarded as a mild form of *folie du doute,* a perpetual self-questioning and uncertainty, however unpleasant it may be from the social point of view, is by no means an unsatisfactory attitude in the face of intellectual problems, for it involves that unceasing self-criticism which is an essential element of all good intellectual work, and has marked more or less clearly the greatest men of scientific genius. Fundamentally, no doubt, timidity is a minor congenital defect of the nervous mechanism, fairly comparable to stammering. It may be noted that the opposite characteristic of over self-confidence, with more or less tendency to arrogance and insolence, is also noted, but with much less frequency, and usually in men whose eminence is not due to purely intellectual qualities. In some cases, it would seem, the two opposite tendencies are combined, the timid man seeking refuge from his own timidity in the assumption of arrogance.

In a certain number of cases information is given as to the general emotional disposition, whether to melancholy and depression, or of a gay, cheerful and genial character. In sixty-two cases the disposition is noted as melancholy, in twenty-nine as cheerful or jovial; in eight cases both dispositions are noted as occurring, in varying association, in the same person. This marked tendency to melancholy among persons of intellectual aptitude is no new observation, but was indeed one of the very earliest points noted concerning men of genius. It was remarked by Aristotle, and Reveillé-Parise, one of the earliest and still one of the most sagacious of the modern writers on genius, devoted a chapter to the point. It is not altogether difficult to account for this phenomenon. Melancholy children, as Marro found, are in large proportion the offspring of elderly fathers, as we have also found our persons of intellectual eminence to be. A tendency to melancholy, again, even though it may always fall short of insane melancholia, is allied to those neurotic and abnormal conditions which we have

* "None are so bold as the timid when they are fairly roused," wrote Mrs. Browning in her 'Letters.' The same point has been brought out by Dugas in his essay on timidity.

found to be not infrequent. Moreover, it certainly has a stimulating influence on intellectual work. The more normal man of cheerful disposition instinctively seeks the consolations of society. The melancholy man, like the shy man, is ill-adapted to society, and more naturally seeks his consolations in a non-social field, such as that of the intellect, often plunging more deeply into intellectual work the more profound his melancholy becomes. Wagner said that his best work was done at times of melancholy, and among the eminent men on our list several writers are mentioned who turned to authorship as a relief to personal depression. It may also be said that not only is melancholy a favorable condition for intellectual work, but that the sedentary and nerve-exhausting nature of nearly all forms of intellectual work in turn reacts to emphasize or produce moods of depression.

There is another cause which serves to explain or to accentuate the tendency of men of genius to melancholy. I refer to the attitude of the world towards them. Every original worker in intellectual fields, every man who makes some new thing, is certain to arouse hostility where he does not meet with indifference. He sets out in his chosen path, ignorant of men, but moved by high ideals, content to work in laborious solitude and to wait, and when at last he turns to his fellows, saying, 'See what I have done for you!' he finds that he has to meet only the sneering prejudices of the few who might have comprehended, and the absolute indifference of the many who are too absorbed in the daily struggle for bread to comprehend any intellectual achievement. The wise worker knows this and arms himself with contempt, as a protection alike against the few and the many;* but it has to be remembered that the prevailing temperament of men of genius is one of great nervous sensitiveness and irritability—so that, as Reveillé-Parise puts it, they are apt to 'roar at a pin-prick'—and even when they are well aware what the opinion of the world is worth, they still cannot help being profoundly affected by that opinion. Hence a fruitful source of melancholy.

The attitude of the world towards the man of original intellect is, however, by no means one merely of disdain or indifference. It constantly tends to become more aggressive. It is practically impossible to estimate the amount of persecution to which this group of preeminent British persons has been subjected, for it has shown itself in innumerable forms, and varies between a mere passive refusal to

* Thus of one of the great men of science on our list, Stephen Hales, it was said that he could look "even upon those who did him unkind offices without any emotion of particular indignation, not from want of discernment or sensibility; but he used to consider them only like those experiments which, upon trial, he found could never be applied to any useful purpose, and which he therefore calmly and dispassionately laid aside."

have anything whatever to do with them or their work and the active infliction of physical torture and death. There is, however, at least one form of persecution, very definite in character, which it is easy to estimate, since the national biographers have probably in few cases passed it over. I refer to imprisonment. I find that over 14 per cent. of these 902 eminent persons were imprisoned, once or oftener, for periods of varying length, while many others only escaped imprisonment by voluntary exile. It is true that the causes of imprisonment were various, but even imprisonment for debt may usually be taken to indicate an anomalous lack of adjustment to the social environment. The man of genius is an abnormal being, thus arousing the instinctive hostility of society, which by every means seeks to put him out of the way.

It will be seen that the various personal traits noted in this section, while completing our picture of British persons of genius, may be linked on at various points to other traits we have previously noted. It only remains to gather together the various threads we have traced and to ascertain how far they may be harmoniously woven into a complete whole.

FREDERIC MYERS'S SERVICE TO PSYCHOLOGY.*

BY PROFESSOR WILLIAM JAMES,

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ON this memorial occasion it is from English hearts and tongues belonging, as I never had the privilege of belonging, to the immediate environment of our lamented President, that discourse of him as a man and as a friend must come. It is for those who participated in the endless drudgery of his labors for our Society to tell of the high powers he showed there; and it is for those who have something of his burning interest in the problem of our human destiny to estimate his success in throwing a little more light into its dark recesses. To me it has been deemed best to assign a colder task. Frederic Myers was a psychologist who worked upon lines hardly admitted by the more academic branch of the profession to be legitimate; and as for some years I bore the title of 'Professor of Psychology,' the suggestion has been made (and by me gladly welcomed) that I should spend my portion of this hour in defining the exact place and rank which we must accord to him as a cultivator and promoter of the science of the mind.

Brought up entirely upon literature and history, and interested at first in poetry and religion chiefly; never by nature a philosopher in the technical sense of a man forced to pursue consistency among concepts for the mere love of the logical occupation; not crammed with science at college, or trained to scientific method by any passage through a laboratory; Myers had as it were to re-create his personality before he became the wary critic of evidence, the skilful handler of hypothesis, the learned neurologist and omnivorous reader of biological and cosmological matter, with whom in later years we were acquainted. The transformation came about because he needed to be all these things in order to work successfully at the problem that lay near his heart; and the ardor of his will and the richness of his intellect are proved by the success with which he underwent so unusual a transformation.

The problem, as you know, was that of seeking evidence for human immortality. His contributions to psychology were incidental to that research, and would probably never have been made had he not entered on it. But they have a value for science entirely inde-

* From the 'Proceedings of the Society for Psychical Research.'

pendent of the light they shed upon that problem; and it is quite apart from it that I shall venture to consider them.

If we look at the history of mental science we are immediately struck by diverse tendencies among its several cultivators, the consequence being a certain opposition of schools and some repugnance among their disciples. Apart from the great contrasts between minds that are teleological or biological and minds that are mechanical, between the animists and the associationists in psychology, there is the entirely different contrast between what I will call the classic-academic and the romantic type of imagination. The former has a fondness for clean pure lines and noble simplicity in its constructions. It explains things by as few principles as possible and is intolerant of either nondescript facts or clumsy formulas. The facts must lie in a neat assemblage, and the psychologist must be enabled to cover them and 'tuck them in' as safely under his system as a mother tucks her babe in under the down coverlet on a winter night. Until quite recently all psychology, whether animistic or associationistic, was written on classic-academic lines. The consequence was that the human mind, as it is figured in this literature, was largely an abstraction. Its normal adult traits were recognized. A sort of sunlit terrace was exhibited on which it took its exercise. But where that terrace stopped, the mind stopped; and there was nothing farther left to tell of in this kind of philosophy but the brain and the other physical facts of nature on the one hand, and the absolute metaphysical ground of the universe on the other.

But of late years the terrace has been overrun by romantic improvers, and to pass to their work is like going from classic to Gothic architecture, where few outlines are pure and where uncouth forms lurk in the shadows. A mass of mental phenomena are now seen in the shrubbery beyond the parapet. Fantastic, ignoble, hardly human, or frankly non-human are some of these new candidates for psychological description. The menagerie and the madhouse, the nursery, the prison, and the hospital, have been made to deliver up their material. The world of mind is shown as something infinitely more complex than was suspected; and whatever beauties it may still possess, it has lost at any rate the beauty of academic neatness.

But despite the triumph of romanticism, psychologists as a rule have still some lingering prejudice in favor of the nobler simplicities. Moreover there are social prejudices which scientific men themselves obey. The word 'hypnotism' has been trailed about in the newspapers so that even we ourselves rather wince at it, and avoid occasions of its use. 'Mesmerism,' 'clairvoyance,' 'medium'—*horrescimus referentes!*—and with all these things, infected by their previous mystery-mongering discoverers, even our best friends had rather avoid complicity. For

instance, I invite eight of my scientific colleagues severally to come to my house at their own time, and sit with a medium for whom the evidence already published in our 'Proceedings' had been most noteworthy. Although it means at worst the waste of the hour for each, five of them decline the adventure. I then beg the 'Commission' connected with the chair of a certain learned psychologist in a neighboring university to examine the same medium, whom Mr. Hodgson and I offer at our own expense to send and leave with them. They also have to be excused from any such entanglement. I advise another psychological friend to look into this medium's case, but he replies that it is useless, for if he should get such results as I report, he would (being suggestible) simply believe himself hallucinated. When I propose as a remedy that he should remain in the background and take notes, whilst his wife has the sitting, he explains that he can never consent to his wife's presence at such performances. This friend of mine writes *ex cathedra* on the subject of psychical research, declaring (I need hardly add) that there is nothing in it; the chair of the psychologist with the Commission was founded by a spiritist, partly with a view to investigate mediums; and one of the five colleagues who declined my invitation is widely quoted as an effective critic of our evidence. So runs the world away! I should not indulge in the personality and triviality of such anecdotes, were it not that they paint the temper of our time, a temper which, thanks to Frederic Myers more than to any one, will certainly be impossible after this generation. Myers was, I think, decidedly exclusive and intolerant by nature. But his keenness for truth carried him into regions where either intellectual or social squeamishness would have been fatal, so he 'mortified' his *amour propre,* unclubbed himself completely, and became a model of patience, tact, and humility wherever investigation required it. Both his example and his body of doctrine will make this temper the only one henceforward scientifically respectable.

If you ask me how his doctrine has this effect, I answer: *By coordinating!* For Myers's great principle of research was that in order to understand any one species of fact we ought to have all the species of the same general class of fact before us. So he took a lot of scattered phenomena, some of them recognized as reputable, others outlawed from science, or treated as isolated curiosities; he made series of them, filled in the transitions by delicate hypotheses or analogies, and bound them together in a system by his bold inclusive conception of the Subliminal Self, so that no one can now touch one part of the fabric without finding the rest entangled with it. Such vague terms of apperception as psychologists have hitherto been satisfied with using for most of these phenomena, as 'fraud,' 'rot,' 'rubbish,' will no more be possible hereafter than 'dirt' is possible

as a head of classification in chemistry, or 'vermin,' in zoology. Whatever they are, they are things with a right to definite description and to careful observation.

I cannot but account this as a great service rendered to psychology. I expect that Myers will ere long distinctly figure in mental science as the radical leader in what I have called the romantic movement. Through him for the first time, psychologists are in possession of their full material, and mental phenomena are set down in an adequate inventory. To bring unlike things thus together by forming series of which the intermediary terms connect the extremes, is a procedure much in use by scientific men. It is a first step made towards securing their interest in the romantic facts, that Myers should have shown how easily this familiar method can be applied to their study.

Myers's conception of the extensiveness of the Subliminal Self quite overturns the classic notion of what the human mind consists in. The supraliminal region, as Myers calls it, the classic-academic consciousness, which was once alone considered either by associationists or animists, figures in his theory as only a small segment of the psychic spectrum. It is a special phase of mentality, teleologically evolved for adaptation to our natural environment, and forms only what he calls a 'privileged case' of personality. The outlying Subliminal, according to him, represents more fully our central and abiding being.

I think the words subliminal and supraliminal unfortunate, but they were probably unavoidable. I think, too, that Myers's belief in the ubiquity and great extent of the Subliminal will demand a far larger number of facts than sufficed to persuade him, before the next generation of psychologists shall become persuaded. He regards the Subliminal as the enveloping mother-consciousness in each of us, from which the consciousness we wot of is precipitated like a crystal. But whether this view get confirmed or get overthrown by future inquiry, the definite way in which Myers has thrown it down is a new and specific challenge to inquiry. For half a century now, psychologists have fully admitted the existence of a subliminal mental region, under the name either of unconscious cerebration or of the involuntary life; but they have never definitely taken up the question of the extent of this region, never sought explicitly to map it out. Myers definitely attacks this problem, which, after him, it will be impossible to ignore.

What is the precise constitution of the Subliminal—such is the problem which deserves to figure in our science hereafter as the *problem of Myers;* and willy-nilly, inquiry must follow on the path which it has opened up. But Myers has not only propounded the problem definitely, he has also invented definite methods for its solution. Post-hypnotic suggestion, crystal-gazing, automatic writing and trance-speech, the willing-game, etc., are now, thanks to him, instruments of

research, reagents like litmus paper or the galvanometer, for revealing what would otherwise be hidden. These are so many ways of putting the Subliminal on tap. Of course without the simultaneous work on hypnotism and hysteria independently begun by others, he could not have pushed his own work so far. But he is so far the only generalizer of the problem and the only user of all the methods; and even though his theory of the extent of the Subliminal should have to be subverted in the end, its formulation will, I am sure, figure always as a rather momentous event in the history of our science.

Any psychologist who should wish to read Myers out of the profession—and there are probably still some who would be glad to do so to-day—is committed to a definite alternative. Either he must say that we knew all about the subliminal region before Myers took it up, or he must say that it is certain that states of super-normal cognition form no part of its content. The first contention would be too absurd. The second one remains more plausible. There are many first-hand investigators into the Subliminal who, not having themselves met with anything super-normal, would probably not hesitate to call all the reports of it erroneous, and who would limit the Subliminal to dissolutive phenomena of consciousness exclusively, to lapsed memories, subconscious sensations, impulses and *phobias,* and the like. Messrs. Janet and Binet, for aught I know, may hold some such position as this. Against it Myers's thesis would stand sharply out. Of the Subliminal, he would say, we can give no ultra-simple account: there are discrete regions in it, levels separated by critical points of transition, and no one formula holds true of them all. And any conscientious psychologist ought, it seems to me, to see that, since these multiple modifications of personality are only beginning to be reported and observed with care, it is obvious that a dogmatically negative treatment of them must be premature, and that the problem of Myers still awaits us as the problem of far the deepest moment for our actual psychology, whether his own tentative solutions of certain parts of it be correct or not.

Meanwhile, descending to detail, one cannot help admiring the great originality with which Myers wove such an extraordinarily detached and discontinuous series of phenomena together. Unconscious cerebration, dreams, hypnotism, hysteria, inspirations of genius, the willing-game, planchette, crystal-gazing, hallucinatory voices, apparitions of the dying, medium-trances, demoniacal possession, clairvoyance, thought-transference—even ghosts and other facts more doubtful—these things form a chaos at first sight most discouraging. No wonder that scientists can think of no other principle of unity among them than their common appeal to men's perverse propensity to superstition.

Yet Myers has actually made a system of them, stringing them continuously upon a perfectly legitimate objective hypothesis, verified in some cases and extended to others by analogy. Taking the name automatism from the phenomenon of automatic writing—I am not sure that he may not himself have been the first so to baptize this latter phenomenon—he made one great simplification at a stroke by treating hallucinations and active impulses under a common head, as *sensory* and *motor automatisms*. Automatism he then conceived broadly as a message of any kind from the Subliminal to the Supraliminal. And he went a step farther in his hypothetic interpretation, when he insisted on 'symbolism' as one of the ways in which one stratum of our personality will often interpret the influences of another. Obsessive thoughts and delusions, as well as voices, visions, and impulses, thus fall subject to one mode of treatment. To explain them, we must explore the Subliminal; to cure them we must practically influence it.

Myers's work on automatism led to his brilliant conception, in 1891, of hysteria. He defined it, with good reasons given, as "a disease of the hypnotic stratum." Hardly had he done so when the wonderfully ingenious observations of Binet, and especially of Janet in France, gave to this view the completest of corroborations. These observations have been extended in Germany, America, and elsewhere; and although Binet and Janet worked independently of Myers, and did work far more objective, he nevertheless will stand as the original announcer of a theory which, in my opinion, makes an epoch, not only in medical, but in psychological science, because it brings in an entirely new conception of our mental possibilities.

Myers's manner of apprehending the problem of the Subliminal shows itself fruitful in every possible direction. While official science practically refuses to attend to Subliminal phenomena, the circles which do attend to them treat them with a respect altogether too undiscriminating—every Subliminal deliverance must be an oracle. The result is that there is no basis of intercourse between those who best know the facts and those who are most competent to discuss them. Myers immediately establishes a basis by his remark that in so far as they have to use the same organism, with its preformed avenues of expression—what may be very different strata of the Subliminal are condemned in advance to manifest themselves in similar ways. This might account for the great generic likeness of so many automatic performances, while their different starting points behind the threshold might account for certain differences in them. Some of them, namely, seem to include elements of supernormal knowledge; others to show a curious subconscious mania for personation and deception; others again to be mere drivel. But Myers's conception of various strata or levels in the Subliminal sets us to analyzing them all from a new point

of view. The word Subliminal for him denotes only a region, with possibly the most heterogeneous contents. Much of the content is certainly rubbish, matter that Myers calls dissolutive, stuff that dreams are made of, fragments of lapsed memory, mechanical effects of habit and ordinary suggestion; some belongs to a middle region where a strange manufacture of inner romances perpetually goes on; finally, some of the content appears superiorly and subtly perceptive. But each has to appeal to us by the same channels and to use organs partly trained to their performance by messages from the other levels. Under these conditions what could be more natural to expect than a confusion, which Myers's suggestion would then have been the first indispensable step towards finally clearing away.

Once more, then, whatever be the upshot of the patient work required here, Myers's resourceful intellect has certainly done a service to psychology.

I said a while ago that his intellect was not by nature philosophic in the narrower sense of being that of a logician. In the broader sense of being a man of wide scientific imagination, Myers was most eminently a philosopher. He has shown this by his unusually daring grasp of the principle of evolution, and by the wonderful way in which he has worked out suggestions of mental evolution by means of biological analogies. These analogies are, if anything, too profuse and dazzling in his pages; but his conception of mental evolution is more radical than anything yet considered by psychologists as possible. It is absolutely original; and, being so radical, it becomes one of those hypotheses which, once propounded, can never be forgotten, but soon or later have to be worked out and submitted in every way to criticism and verification.

The corner-stone of his conception is the fact that consciousness has no essential unity. It aggregates and dissipates, and what we call normal consciousness—the 'Human Mind' of classic psychology—is not even typical, but only one case out of thousands. Slight organic alterations, intoxications and auto-intoxications, give supraliminal forms completely different, and the subliminal region seems to have laws in many respects peculiar. Myers thereupon makes the suggestion that the whole system of consciousness studied by the classic psychology is only an extract from a larger total, being a part told off, as it were, to do service in the adjustments of our physical organism to the world of nature. This extract, aggregated and personified for this particular purpose, has, like all evolving things, a variety of peculiarities. Having evolved, it may also dissolve, and in dreams, hysteria, and divers forms of degeneration it seems to do so. This is a retrograde process of separation in a consciousness of which the unity was once effected. But again the consciousness may follow the opposite course

and integrate still farther, or evolve by growing into yet untried directions. In veridical automatisms it actually seems to do so. It drops some of its usual modes of increase, its ordinary use of the senses, for example, and lays hold of bits of information which, in ways that we cannot even follow conjecturally, leak into it by way of the Subliminal. The ulterior source of a certain part of this information (limited and perverted as it always is by the organism's idiosyncrasies in the way of transmission and expression) Myers thought he could reasonably trace to departed human intelligence, or its existing equivalent. I pretend to no opinion on this point, for I have as yet studied the evidence with so little critical care that Myers was always surprised at my negligence. I can therefore speak with detachment from this question and, as a mere empirical psychologist, of Myers's general evolutionary conception. As such a psychologist I feel sure that the latter is a hypothesis of first-rate philosophic importance. It is based, of course, on his conviction of the extent of the Subliminal, and will stand or fall as that is verified or not; but whether it stand or fall, it looks to me like one of those sweeping ideas by which the scientific researches of an entire generation are often moulded. It would not be suprising if it proved such a leading idea in the investigation of the near future; for in one shape or another, the Subliminal has come to stay with us, and the only possible course to take henceforth is radically and thoroughly to explore its significance.

Looking back from Frederic Myers's vision of vastness in the field of psychological research upon the programme as most academic psychologists frame it, one must confess that its limitation at their hands seems not only unplausible, but, in truth, a little ridiculous. Even with brutes and madmen, even with hysterics and hypnotics admitted as the academic psychologists admit them, the official outlines of the subject are far too neat to stand in the light of analogy with the rest of nature. The ultimates of nature—her simple elements, if there be such—may indeed combine in definite proportions and follow classic laws of architecture; but in her proximates, in her phenomena as we immediately experience them, nature is everywhere gothic, not classic. She forms a real jungle, where all things are provisional, half-fitted to each other, and untidy. When we add such a complex kind of subliminal region as Myers believed in to the official region, we restore the analogy; and, though we may be mistaken in much detail, in a general way, at least, we become plausible. In comparison with Myers's way of attacking the question of immortality in particular, the official way is certainly so far from the mark as to be almost preposterous. It assumes that when our ordinary consciousness goes out, the only alternative surviving kind of consciousness that could be possible is abstract mentality, living on spiritual truth, and

communicating ideal wisdom—in short, the whole classic platonizing Sunday-school conception. Failing to get that sort of thing when it listens to reports about mediums, it denies that there can be anything. Myers approaches the subject with no such *a priori* requirement. If he finds any positive indication of 'spirits,' he records it, whatever it may be, and is willing to fit his conception to the facts, however grotesque the latter may appear, rather than to blot out the facts to suit his conception. But, as was long ago said by our collaborator, Mr. Canning Schiller, in words more effective than any I can write, if any conception should be blotted out by serious lovers of nature, it surely ought to be the classic academic Sunday-school conception. If anything is *un*likely in a world like this, it is that the next adjacent thing to the mere surface-show of our experience should be the realm of eternal essences, of platonic ideas, of crystal battlements, of absolute significance. But whether they be animists or associationists, a supposition something like this is still the assumption of our usual psychologists. It comes from their being for the most part philosophers in the technical sense, and from their showing the weakness of that profession for logical abstractions. Myers was primarily a lover of life and not of abstractions. He loved human life, human persons, and their peculiarities. So he could easily admit the possibility of level beyond level of perfectly concrete experience, all 'queer and cactus-like' though it might be, before we touch the absolute, or reach the eternal essences.

Behind the minute anatomists and the physiologists, with their metallic instruments, there have always stood the out-door naturalists with their eyes and love of concrete nature. The former call the latter superficial, but there is something wrong about your laboratory-biologist who has no sympathy with living animals. In psychology there is a similar distinction. Some psychologists are fascinated by the varieties of mind in living action, others by the dissecting out, whether by logical analysis or by brass instruments, of whatever elementary mental processes may be there. Myers must decidedly be placed in the former class, though his powerful use of analogy enabled him also to do work after the fashion of the latter. He loved human nature as Cuvier and Agassiz loved animal nature; in his view, as in their view, the subject formed a vast living picture. Whether his name will have in psychology as honorable a place as their names have gained in the sister science, will depend on whether future inquirers shall adopt or reject his theories; and the rapidity with which their decision shapes itself will depend largely on the vigor with which this Society continues its labor in his absence. It is at any rate a possibility, and I am disposed to think it a probability, that Frederic Myers will always be remembered in psychology as the pioneer who staked out a vast tract of

mental wilderness and planted the flag of genuine science upon it. He was an enormous collector. He introduced for the first time comparison, classification, and serial order into the peculiar kind of fact which he collected. He was a genius at perceiving analogies; he was fertile in hypotheses; and as far as conditions allowed it in this meteoric region, he relied on verification. Such advantages are of no avail, however, if one has struck into a false road from the outset. But should it turn out that Frederic Myers has really hit the right road by his divining instinct, it is certain that, like the names of others who have been wise, his name will keep an honorable place in scientific history.

THE POSE OF THE BODY AS RELATED TO THE TYPE OF THE CRANIUM AND THE DIRECTION OF THE VISUAL PLANE.*

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NEW YORK.

IT is a novel proposition that the position of the head in respect to the body or of the shoulders in reference to the back, that the carriage of the whole body in walking and the attitude of a person in conversation, should be governed in an important measure by the form of the cranium. It will also, doubtless, be regarded as a bold assertion to say that all these positions and attitudes and even the gait of individuals are largely modified, even in many instances controlled, by the normal position of the eyes in respect to the cranium. Yet it is not difficult to show that both these propositions are true and that the truth contained in these statements is not only of importance as a principle in art, but that it is of great practical value from the point of view of the well-being of large classes of persons.

From the standpoint of art the principle involved in these propositions shows the error of representing individuals who have certain forms of crania in attitudes which, for persons with those special cranial characteristics, would be unnatural and almost absurd. For example, when Thorwaldsen represented Sir Walter Scott with his chin elevated high in the air, he gave to the distinguished author of Waverley a posture of the head which would have been not only painful, but almost impossible for him to have maintained as a characteristic pose.

From the more practical and more important standpoint it may even be said that, owing to the position of the visual plane in respect to the head, there may be comparative immunity from certain complaints and diseases or a comparative predisposition to those very affections according to the type of the head and the direction of the normal plane of vision.

In order to facilitate the examination of the topic, it will be necessary to define some of the terms and some of the principles which enter into it.

The term 'normal plane of vision' will be used, and it should at the outset be understood exactly what is meant by the phrase.

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* An address delivered before the Section of Anthropology of The American Association for the Advancement of Science, at its session at Yale University, New Haven, December 27th, 1899.

By the term 'normal plane of vision,' as here employed, it is intended to express the direction which the lines of sight would assume, the head being in the erect, or more technically in what is known as the *primary*, position, in case no muscular effort should be made to change the eyes from an entirely passive adjustment.

Under these conditions the line of sight of each of the two eyes lies in an imaginary plane which may be coincident with, or somewhat higher than, or somewhat lower than, the plane of the horizon.

The *normal plane of vision* differs essentially from the *primary plane of regard* of Helmholtz and other writers, for this plane of regard refers to the plane formed by the two visual lines when these lines are directed toward the horizon, the head being in the primary position. The plane of regard is therefore unalterable. It was my own privilege to show that the normal plane of vision not only varies in different individuals but that, as a general rule, this variation is associated with and controlled by certain cranial characteristics which will presently engage our attention.*

This leads us to the consideration of certain types of the human cranium as they are recognized by craniologists.

While the form of the head of an individual may not be so clearly of one or another type that it must be classified as belonging to a certain group, in general, heads are grouped into three great classes or types, and these classes or types are again divided into subtypes. In this connection the subtypes need not be taken into consideration, but some knowledge of the main types is essential.

Craniologists, then, classify crania as *long, broad* and *medium*. Medium skulls, in order to avoid misconception, will be here designated as *tall* skulls, since the term medium does not, in this relation, refer to capacity, but to certain special measurements, and the accepted term might be misleading to those not well versed in the subject.

The basis for the classification consists of the proportion which the longest diameter from before backwards bears to the longest transverse diameter. If the transverse diameter is $^{75}/_{100}$ that of the longer diameter or less than $^{75}/_{100}$, the head is said to be in the class of long heads; but if the transverse diameter equals or exceeds $^{85}/_{100}$ the length of the skull, it is a broad skull. Medium skulls, or, as we are now to call them, tall skulls, are those in which the transverse diameter is between $^{75}/_{100}$ and $^{85}/_{100}$ and, as might be supposed, the measurement from the base of the skull to its summit, while it may not of itself be greater in an individual case than that of one of the long or one of the broad type nor even so great, is greater in proportion to the other measurements.

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* The Normal Plane of Vision in Relation to Certain Cranial Characteristics. 'Archives of Ophthalmology,' Vol. xxvi, No. 3, 1897.

The diagrams, Figs. 1, 2 and 3, give the general outline of the form of heads of these types when looked at from above.

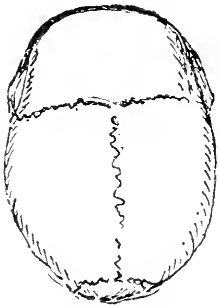


FIG. 1. THE LONG HEAD.

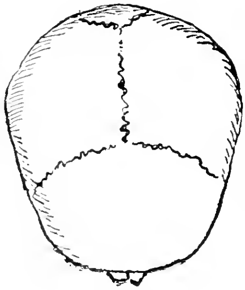


FIG. 2. THE TALL HEAD.

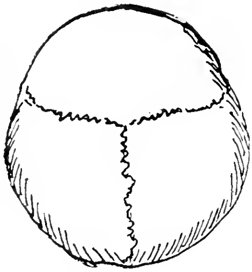


FIG. 3. THE BROAD HEAD.

In typical heads belonging to either of these types the outline of the face is likely to be characteristic of the type. Thus, the general outline of the face from the line of the brows to the tip of the chin as seen from the side differs, as a rule, according to the type of the cranium. Associated with the long cranium there is generally a convex facial outline, while a side view of the face of one from the class of tall heads shows usually very little or no curve. On the other hand the face of one from the class of broad skulls is likely to show a concave line.

The next series of figures, 4, 5 and 6, gives an idea of the general form from a side view of each of these three types in the living subject.

FIG. 4. THE LONG HEAD.
FACIAL ANGLE + 10°.

FIG. 5. THE TALL (MEDIUM) HEAD.
FACIAL ANGLE 0°.

FIG. 6. THE BROAD HEAD.
FACIAL ANGLE — 10°.

To nearly all general laws affecting the form of the human body there are exceptions, and the rule just stated is not absolutely uniform in its application. However, the type of head and the outline of face are generally in the relation shown by the diagrams.

In these three classes of crania the normal visual plane does not as a rule occupy the same position in relation to the horizontal plane, but varies according to the type of skull.

The relation of the normal visual plane to the type of tne cranium in each of the three classes may be arrived at by direct and by indirect methods.

In the case of the living subject, the dimensions of the head may be taken and the plane of vision established in the same individual. The determination of the plane of vision in the living subject is accomplished through the aid of an instrument known as the tropometer. The relation is thus established by a direct method. The indirect method is that of ascertaining the direction of the imaginary line constituting the axis of the orbit in the prepared skull, the measurements of which are known. The orbits are more or less cone-shaped. If the extreme apex of the cone, at which the optic nerve enters it, is taken as one point of the line of the axis, and a point where two straight lines drawn at nearly right angles with each other from certain parts of the circle of bone constituting the outer border of the orbit cross is taken as another point in the line of the axis, the line which would pass through these two points would represent the axis. This imaginary line, if projected forward and beyond the orbit, would be seen in most cases to point somewhat downward, the skull being in the primary position, and in some types of skulls it points much more downward than in other types.

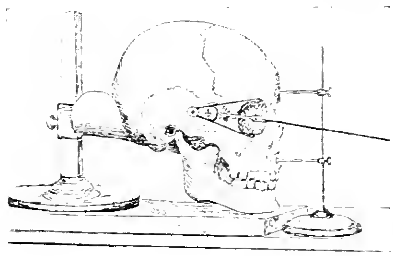


FIG. 7. THE AUTHOR'S METHOD OF DETERMINING THE AXIS OF THE ORBIT.

It is interesting to find that the pointing of the imaginary line representing the axis of the orbit closely corresponds with the observations on the normal visual plane in the living subject.

The interest is more considerable when it is found that the form of the orbit in the different classes of skulls offers an explanation of the peculiarities in the direction of the orbital axis, as well as of the normal plane of vision.

Figs. 8, 9 and 10 represent the front views of skulls of the long, tall and broad types respectively, showing the form of the orbit corresponding to each type. It will be seen that in the long skull (Fig. 8) the roof of the orbit is much lower than that of the tall skull (Fig. 9) and that the lower border extends more downward. The orbit of the tall skull is not only placed with its opening higher, but it is more narrow from side to side. In the case of the broad skull (Fig. 10) the

roof of the orbit is low like that of the long skull, but the lower border does not extend so far downward and the direction of the transverse diameter is more oblique.

Measurements of the direction of the axis of the orbit in these three classes show that in the long skull the direction is usually quite

Fig. 8.
Front view of Long Skull: Cephalic Index 71.4 : 100.

Fig. 9.
Front view of Tall Skull: Cephalic Index 81 : 100.

Fig. 10.
Front view of Broad Skull: Cephalic Index 85 : 100.

low, that in the tall skull it is much higher and that, while the axis of the broad skull is lower than that of the tall one, it is scarcely as low on the average as in the case of the long skull; and these comparative positions of the axes of the orbits in the prepared skulls correspond remarkably with the positions of the visual plane in the case of living subjects with heads of corresponding types. That is, the visual plane of the long head is low, of the broad head also low and that of the tall head is high.

Notwithstanding the apparent simplicity of these relations of the form of the orbit with the type of the skull and of the direction of the visual plane to the type of cranium, there are, in practice, certain modifying features.

The most important of these from the anatomical standpoint is found in the angle of the face. In forming a judgment, therefore, of the probable direction of the normal visual plane in the living subject without resorting to measurements by the tropometer, it is necessary to measure or to estimate this angle. None of the measurements employed for the prepared skull will serve the purpose here, and it has been found most practical to use for fixed points for the measurement of this angle the following: the glabella, which is the elevation above the root of the nose and just between the ridges of bone above the orbits; the depression just below the nose, and the tip of the chin.

If these three fixed points are selected for the measurement of the angle of the face, it will readily be seen that this angle varies greatly in different individuals. It may, indeed, vary considerably in heads belonging to the same type. Yet, on the whole, there is a pretty

general association between the character of the face and the type of the head.

In the case of the long head, for example, the angle is external, as will be seen on turning to the diagram, Fig. 4, in which the facial angle is plus (+) 11°, or to Fig. 14 where it is plus (+) 15°. In the broad head, on the contrary, the angle is likely to be inverse, as in Fig. 6 where the angle is minus (—) 10°. In the tall head, however, the facial angle almost vanishes. It is, in general, 0° as in Fig. 5 or only an angle of from + 2° to + 4°, rarely exceeding + 6°. But, as already intimated, the angle may vary in each type of head.

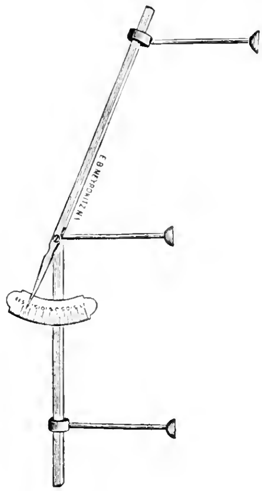


FIG. 11. STEVENS'S FACIAL GONIOMETER.

Now, if the angle of the face is taken in connection with the type of head, we have a fairly certain indication of the direction of the plane of vision.

With the long head and strong external facial angle the plane of vision is almost invariably low. With the broad head and inverse angle the visual plane is also low, but there is a restricted downward range of the rotations of the eyes, notwithstanding this depressed position of the visual plane. With the tall head and straight face the plane of vision is high and in proportion as the head is comparatively tall it may be very high.

When these elemental principles are once understood it is not difficult to comprehend the phenomena to which they give rise.

Thus it is easy to see that a person whose normal plane of vision is quite low finds it easier to throw the head backwards, lifting the chin and forcing the forehead back, than to raise the visual plane to the level of the horizon or even to the lower plane which the eyes assume in walking, if that visual plane has to be thus elevated and the elevation maintained for some time by the delicate muscles which act directly in elevating the eyes.* On the contrary one whose visual plane is very high prefers to throw the forehead in advance and the chin into the breast rather than maintain a tension upon the little muscles which act directly upon the eyes to pull them down.

Thus it will be seen that the person represented at Fig. 12, with the long head (from before backward) and the strong angle of the face, carries the forehead quite far back and the chin well up, not from

* There is more in this statement than would at first appear, for the important question of the horopter must be included here, but this would add an element too extensive for present discussion.

any affectation of attitude, but because it is less wearisome to the eyes to assume this position. As a matter of fact this person's eyes were normally adjusted 10° below the plane which has been found to be the

FIG. 12.

FIG. 13.

best and which may be called the standard plane. On the contrary the person whose pose is represented at Fig. 13, whose head is high compared to its transverse and horizontal diameters, a head which is neither of the long nor broad type, but of the medium (tall) type with the absence of a strong angle of the face, had the plane of vision very high. Such a person prefers to throw the forehead in advance and the chin into the breast, rather than make a continual and somewhat tiresome effort to draw the eyes to the proper plane by direct tension upon the depressor muscles of the eyes.

It is not difficult to see that this selection of the easiest method of adjusting the lines of sight to surrounding objects exercises a commanding influence on the whole pose of the body.

While the rule generally holds that the form of the skull and therefore the form of the orbit governs the direction of the visual plane and hence also the pose of the head and of the body, there are other elements which enter into the case and give rise to exceptions. The most important of these modifying elements is the condition known as the 'declination of the vertical meridians of the retina';* still, in order

* The subject of the horopter has already been referred to in the note to page 395. In regard to declinations, while it would be impossible to enter upon that complex subject here, it would be misleading were we to pass it by without the statement that the pose of the head is sometimes governed, even against the rule which it is sought here to point out, by the direction of the retinal meridians and hence in practice a knowledge of this subject would be essential to a full understanding of the subject under discussion.

to avoid taking too wide a range of discussion, we shall treat the subject as though the influence of the plane of vision were in all cases uniform, which is not strictly the case; yet, for our present purpose, we may omit the exceptions, and the statements that follow must be accepted as general and as including the proviso, other things being equal.

Examining then the pose of the head and body in their relations to the position of the visual plane more in detail, it will be found that when the visual plane is quite low not only is there a tendency for the individual to elevate the chin and throw the head back, but the muscles of the whole back, even those of its lower part, are put in a state of tension. This tension is so considerable that in a great many instances among persons who are not very strong the resulting habitual pains of the muscles engaged are often mistaken and treated for diseases of internal organs or of the spine year after year.

An element of the facial expression with this direction of the visual plane is the pronounced elevation of the brows upon the forehead and the somewhat drooping appearance of the eyelids (see Fig. 4). There are other facial expressions characteristic of this depressed plane of vision which may be passed over in this connection.

The attitude and gait of the individual are also generally influenced by the downward direction of the visual plane. In walking the shoulders are thrown back and the chest is thrust forward. The foot, in many cases, swings forward considerably beyond the limit of the completed step, so that it is drawn somewhat backward as it comes to the ground.

While persons of this class are more liable than others to certain physical complaints and nervous disturbances which can be traced directly to this ocular condition than are those whose eyes are adjusted for a higher plane, they are, on the other hand, compared with this latter class far less subject to certain other forms of diseases and affections.

A single example will serve to illustrate this proposition. Although the illustration relates to a trouble with the eyes themselves it would be easy to present many examples to illustrate immunity of the same class of persons from a variety of more general affections.

A few years since a distinguished oculist of one of our southern cities announced that trachoma, that form of eye trouble commonly known as granular lids, and which is one of the prolific sources of blindness, is unknown among pure negroes. The discussion of this proposition, after occupying the attention of oculists for some time, was at length taken up in a different way by a distinguished colleague in Constantinople.

This gentleman wrote to oculists in all parts of the world asking

for the results of their observations in their own countries in regard to all classes of people. He at length published a symposium of the answers showing the prevalence of trachoma in different countries and among the different classes of people. As given in this contribution there seemed to be a confused accumulation of facts which had, on the whole, apparently little meaning. Peoples of contiguous countries, of the same color and not very different in habits of life, were reported as differing widely in respect to the prevalence of the affection. No reasons were assigned and none seemed to be suggested by the varying facts. An analysis which I made of this report showed that among peoples with the 'medium' or tall heads, like the Irish and the Italians, trachoma is rife; while among peoples with the broad head, like the Bavarians, or with the long head, like the negroes whose ancestors were from the West or Guinea coast of Africa, trachoma did not prevail; but it is interesting to note that descendants of the negroes of the northern part of Africa, where the heads of the natives are often tall, are subject to trachoma equally with the whites among whom they live. I have in another connection discussed this question at more length.*

FIG. 14. THE LONG HEAD WITH PROGNATHOUS FACE. FACIAL ANGLE + 15°.

A glance at Fig 14 will show that the negro as he is known in our Southern States not only throws the head backward in the manner characteristic of the long head, the strong facial angle and the depressed visual plane, but that the eyebrows are characteristically elevated. This drawing up of the brow is accompanied with a drawing upon the lids and hence no pressure is brought upon the surface of the eyes by the upper lids. In the case of the tall head with the high plane of vision the brows are strongly compressed and the lids bind upon the eyeball and thus in the midst of dust and filth or even in good sanitary surroundings disease of the lids may be promoted.

Reverting once more to the declinations of the retinal meridians, the same effect may be induced both as to the pose of the head and the relaxed state of the lids. It is, however, impracticable to consider the subject from that point of view at present.

Turning to a larger and more important subject, the negro is known to be especially subject to tubercular diseases, yet he is, to an unusual degree, immune from consumption. In my investigations during the past few years, I have not seen a consumptive the direction of whose visual plane was not much higher than the standard. We shall come to this from another point of view. It is easy to see that

* 'Transactions of the British Medical Association,' 1897.

the principle which applies in the case of the long skull applies similarly also in that of the broad skull although, in general, in somewhat less degree.

People who have broad heads with inverse angle of the face usually carry the head with the forehead thrown back and the chin elevated. Those who have this form of head and this consequent depression of the visual plane often suffer from the neuralgic or myalgic affections of the back of the head and the spinal region like the class with the long head and strong exterior facial angle. The differences need not be discussed here.

Directing our attention now to the excessive upward direction of the plane of vision which is found principally with the tall, or more correctly and technically, the mesocephalic head, we find not only a great difference in the adjustments of the facial muscles as compared with the class which we have just considered, but also in the poise of the body. In the class now to be considered the brows are compressed and the expression is one of intensity. The chin is not elevated as in the other class, but the forehead is advanced and the body leans forward. The shoulders bend forward and the chest is often compressed. With the noblest form of the head comes a stoop of the body. Fortunately for the world these people do not all have consumption, for if they did one of the highest forms of development of humanity would be wiped out. Unfortunately, however, it is from this class of people that consumption finds the great majority of its victims. Glance at the position of the air passages in these two portraits in each of which the habitual pose of the body and head is fairly represented.

Fig. 15.

Fig. 16.

In the case of the one with the broad head and difficult upward rotations of the eyes (Fig. 15), a swarm of tubercle bacilli would pass in and out of the respiratory passages with much the same effect as any

other minute particles of dust, while in the case of the tall headed boy (Fig. 16) who has, by actual measurement, the visual plane adjusted more than twenty degrees above the horizon, the larynx forms a hinge-like valve and in the quiet eddies of a lung under these circumstances the tubercle bacilli can easily hold high carnival. If the direction of the large branches of the air tubes is considered it is evident that the circulation of the air in the very upper portions of the lungs of one with such a habitual pose would naturally be even less active than in the lower parts, and it is interesting to remember that it is in the upper lobes that the bacilli usually commence their inroads. The modern treatment of consumption is fresh air. It is evident that the amount of air admitted to the lungs of a person with the habitual attitude of this boy must be very materially modified by this position of the head; and could the normal pose be improved he would by that means be subjected to the fresh-air treatment. It will be seen that this is entirely practicable.

The comparative immunity of the negro race from consumption has already been referred to, and it is a fact of much practical interest that among the people of Iceland, people with the extreme broad head and with the characteristic pose, the head thrown back, the chin elevated, consumption is unknown. Yet these people habitually inhale the most vitiated atmosphere, an atmosphere which, habitually inhaled by the people of this country, would induce an epidemic of consumption which would be of the most devastating character.

As the general pose of the head and the attitude of the body differ in the class with high heads from those of the other classes, so the gait and carriage of these people differ widely from those of the others. If the cake walk is an extravagant exaggeration of the walk of the classes with the low visual plane, the stoop of Shylock as it is represented on the stage is the exaggeration of the carriage of the other class.

This is but a most cursory glance at a most important subject; and the affections and characteristics mentioned are but a group of those whose name is legion and which have for their cause one or other of these visual conditions.

Several years ago attention was called by the writer of this article to the one-sided carriage of the head of those who have the visual line of one eye higher than that of the other. I will only refer to it here as one of the elements in this interesting subject.

The practical question which arises from the presentation of this subject is: Can the direction of the visual axes be so modified in the individual case as to change the pose of the head and body so as to relieve the person from the results of his physical peculiarity? To this an emphatic affirmative answer can be given. The eyes can be adjusted for any desired plane by a safe and speedy procedure. It may be said

that, in the hands of one specially skilled in this procedure, the inconvenience and risk are scarcely as great as those resulting from vaccination as commonly practised.

Thus, by practical and judicious methods, the unfortunate positions of the eyes due to the hereditary form of the orbits may be rectified, and the handicap with which large classes of persons enter upon the course of life may be removed, thereby offering them that advantage and that staying quality in the contest for welfare and for success to which, by their native strength and inborn abilities, they are fairly entitled.

THE GREAT MORTALITY.

BY PROFESSOR EDWARD P. CHEYNEY.

IN every city of the civilized world to-day, armed and watchful men are standing on guard against a dreaded invader; men armed with knowledge obtained from scientific investigation, with experience drawn from former attacks, with authority of law to enter every household and set aside every individual claim in the work of resistance to the first onset of the foe. This anticipated enemy is the bubonic plague, and the officials of boards of health form a civic guard against it more nearly impassable than any military cordon. Yet with all this watchfulness, with the expenditure of vast sums in delimitation and extirpation, with the relatively cleanly surroundings of daily life in this century, the plague has within the last five years broken through the barriers and made its way into various cities of Asia, Africa, Australia, Europe and North and South America. Not only in its indigenous home in India, but in Sydney and Honolulu, in Lisbon and Rio de Janeiro, in Glasgow and San Francisco, in Cairo and Cape Town, it has made a longer or shorter lodgment.

There was a time when there was no such guard against invasion, when the same disease passed westward from its Asiatic birthplace in a fierce attack upon the nations of Europe, and found no measures taken to resist its advance; indeed, in the squalid houses and streets of medieval towns and villages, there was every inducement to enter and batten on populations unfitted by habits of life or by medical knowledge to expel, resist or even mollify their enemy.

"In the year of grace 1349," an old chronicle says, "a great mortality of mankind advanced over the world; beginning in the regions of the north and east and ending with so great a destruction that scarcely half of the people remained. Then towns once full of men became destitute of inhabitants, and so violently did the pestilence increase that the living were scarce able to bury the dead." So sudden, so mysterious, so fatal was this pestilence that even the dry medieval annalist personified it, spoke of it as if it were some active sentient personality. "Very many of those who were attacked in the morning it carried out of human affairs before noon; and no one whom it willed to die did it permit to live longer than three or four days."

Friar Clyn, in his 'Chronicle of Ireland,' after giving many details of the plague, says: "I, therefore, brother of John Clyn of the order of

the Minors and the convent of Kilkenny, have written down in this book these wonderful occurrences of our time as I have seen them with my eyes or heard them from credible witnesses; and lest such strange things should perish with the passage of time and should pass from the memory of men who are to come, watching these many evils and the whole world fallen into sickness, and waiting among the dead till death shall come, I have put into writing what I have heard truthfully and observed carefully. And lest the writing should perish with the writer and the labor should fail with the laborer, I leave parchment to continue the work, if it should chance that any man should survive, or any of the race of Adam succeed in escaping this pestilence, to continue the work which I have begun." Then follow two or three confused sentences, when his expectation of death must have been justified, for there is nothing more of the chronicle except an annotation by a later hand, *videtur quod auctor hic obiit,* 'it seems that the author here died.' Another chronicler lays down his pen at the onset of the plague, and long afterward when resuming his narrative, sick at heart, perhaps, or feeling his skill inadequate to the description of such a period of confusion, enters in the appropriate place only the words *magna mortalitas,* 'the great mortality.'

This great mortality came 'from the north and east.' On the confines of Asia and Europe, at the mouth of the Sea of Azov, lay the medieval trading city of Kaffa. Here goods were brought from Persia, from India and from China to be handed over by men of the east to men of the west. Genoese and Venetian bought from Tartar and Arab silk, cotton, spices, precious stones and metals, gums, woods and sugar, and carried them through the Black Sea and the Mediterranean to be distributed finally among all the countries of Europe. In the year 1347 a war broke out in the Crimea between these men of the west and of the east, and the Italian inhabitants of Kaffa were besieged by the Tartars. In the midst of the hostilities a terrible pestilence broke out among the besiegers, which devastated their hordes like the hand of the destroying angel in the camp of the Assyrians. In their frenzy the survivors threw numbers of the bodies of those who had died of the plague from their catapults into the besieged city and thus carried infection to those within. The siege was soon raised, and the Genoese merchants and sailors, resuming their trading, set sail toward the west. But they took with them on their voyage, along with the luxuries from the far east, a new scourge for Europe, the contagion of the plague—the Black Death, as it has been called in modern times.

The symptoms of the disease were obscure and varying, and so remained through successive attacks, until only too abundant opportunities for observation have recently enabled modern medical observers to

differentiate its various types. But it showed itself then, as it still remains, fatal in a greater proportion of cases than any other disease to which humanity is subject.

The distribution of its germs through the Mediterranean lands was quickly accomplished. In the fall of 1347 Constantinople was more than decimated. The shores of the Ægean were quickly infected, and before the close of the year Sicily, the cities and towns of southern Italy, and all the ports of the Adriatic were alike prostrate under the scourge. A Sicilian tells how "a most deadly pestilence sprang up over the entire island. It happened that in the month of October, in the year of our Lord 1347, about the beginning of the month, twelve Genoese ships flying from the divine vengeance which our Lord for their sins had sent upon them, put into the port Messina, bringing with them such a sickness clinging to their very bones that, did anyone speak to them, he was directly struck with a mortal sickness from which there was no escape. Flight profited nothing, for the sickness already contracted and clinging to the fugitives was only carried wherever they sought refuge. Some of those who fled fell on the roads and dragged themselves to die in the fields, the woods, the valleys."

By the springtime the storm had spent its fury in the south of Italy, but it had passed on northward. It was in April of 1348, Boccaccio tells us, that the malady appeared in the fair city of Florence. There while human nature was resolved into its most primitive elements, as he describes in the introduction to the Decameron, his little group of story tellers gathered in a country house about two miles outside of the city trying to avoid the pestilence, or at least to make what time should remain pass more cheerfully in the recounting of sad or merry tales. The occasional pathos, the frequent salacity, and the unvarying humor and grace of the tales stand out boldly in Boccaccio's setting of them against the dark background of the mournful remembrance of that most fatal plague so terrible yet in the memories of us all. In the city the sick were lying deserted by friends, family, servants, physicians and even by the priest, as implacable death crept upon them; palaces stood deserted and unfastened, jewels and rich garments lying unguarded, except by the dread of infection; the bodies of the dead were being hastily dragged from the houses, carried to the cemeteries and deposited in long rows in pits, with no bells rung, no rites said, no solemn chant or mourning of friends; while outside the city the story tellers of the Decameron were passing away the time governed by the one rule that none should bring to them any news of the plague-stricken outer world.

Not only Boccaccio in Florence, but Petrarch in Parma, writes in the midst of the plague: "Where are now our pleasant friends? Where the loved faces? Where their cheering words? Where their

sweet and gentle conversation? We were surrounded by a crowd of friends; now we are almost alone." And he might well see the world darkening around him, for had not Laura just died of the plague at Avignon? The 'great mortality' was indeed no respecter of persons. It is true that the poor died in greater numbers in proportion to the closeness and insalubrity of their dwellings and to their lack of power of resistance from insufficient food. But the list of the great ones of the earth who died is a long one. One of the earliest victims of the plague on its entrance into Europe was Andronicus, the son of the Emperor at Constantinople. The King and Queen of Arragon both died from it. Joan, the daughter of Edward III., on her way to Castile to be married, was smitten suddenly at Bordeaux and died, escaping, it is true, the worse fate of living to be the wife of Pedro the Cruel. Great churchmen died in all parts of Europe—the archbishop of Cantania, the archbishop of Canterbury, the archbishop of Drontheim: bishops and abbots in every country. Of the twelve city magistrates of Montpellier, in France, ten died; of the twenty-four prominent physicians, twenty. Nobles and burghers, ecclesiastics and lawyers fared but little better than the great masses, except that their names are mentioned, while the hundreds of thousands of lesser men died unknown.

The Mediterranean was then still the middle of the world, and the pestilence, like art and literature and money, was distributed readily from Italy to all parts of Europe. Before the year 1348 was over it was in France, Switzerland and Germany, and had obtained a foothold in the southern seaports of England. The next year it passed still further northward through England, Scotland and Ireland. It was carried from England to Scandinavia by a London ship, all of whose crew died, leaving the boat with its fatal cargo to be cast on the Norwegian coast at Bergen. During the same year and the succeeding spring, it had passed down the Valley of the Rhine, through the Netherlands and northern Germany, until the year 1350 saw 'the great mortality,' its harvest reaped for that season, passing out of the northern and western portals of Europe to the all-purifying waters of the great ocean.

But during those four years of devastation what experiences had humanity gone through! We can look back now and see only dimly through the mist. The figures are blurred and their movements indistinct. The light of imagination fails to illuminate a condition so different from normal experience. Only here and there a clear light is cast upon some spot by a record made at the time. In the inn of a little town in Spain a French pilgrim returning from the tomb of St. James of Campostella, after supping with the host, who with two daughters and one servant had alone so far survived of his entire family and

who was not then conscious of any sickness upon them, settled with him for his entertainment, intending to start on his journey at daybreak, and went to bed. The next morning, rising and wanting something from those with whom they had supped, the travelers could make no one hear. Then they learned from an old woman whom they found in bed that the host, his two daughters and servant had died in the night. "In the spring of 1348 a Genoese, infected with the plague, came to Piacenza. He sought out his friend, Fulchino della Croce, who took him into his house. Almost immediately afterward he died, and the said Fulchino was also quickly carried off with his entire family and many of his neighbors." "The bishop of Rochester out of his small household lost four priests, five gentlemen, ten serving men, seven young clerks and six pages, so that not a person remained who might serve him in any office." "Alas, for our sorrow! This mortality swept away so vast a multitude of both sexes that none could be found to carry the corpses to the grave. Men and women bore their own offspring on their shoulders to the church and cast them into a common pit." "And I, Agniolo di Tura, carried with my own hands my five little sons to the pit."

Everywhere through Europe the story is the same. The sudden onset, the four or five months of devastation, the glutting of the old cemeteries and the opening up of new, the pits into which the bodies were piled, the extermination of whole families, the collapse of government, the desertion of the sick and the dead, the avoidance of the infected, the occasional heroism, self-sacrifice, devotion to duty, and the sadly more frequent selfishness, cruelty, callousness and recklessness; then the passing away of the visitation, leaving behind it, often, according to the bewildered judgment of the contemporary chronicler, not a tenth part of the people, and even according to modern and moderate computation seldom as many as one-half of the population it had found.

Then came the gleaning. Physical disease then as always brought moral degeneracy. A great catastrophe then as always weakened the virtues and strengthened the vices of poor custom-bound humanity. A physician at Avignon writes: "The father did not visit his son, nor the son his father. Charity was dead." Villani says of his neighbors at Florence that they behaved as "might perhaps be expected from infidels and savages." "Men gave themselves up to the enjoyment of the worldly riches to which they had succeeded." The English manor court rolls record more than one case where a house bereft of its occupants by the plague was plundered by the neighbors, and bodies of the dead stripped by their own fellow villagers. The wealthy, in the months following the plague, gambled, reveled, steeped themselves in gluttony and lechery; the poor idled, brawled, took advantage of the

necessities of their lords, and became irreligious and rebellious. Scarcely a writer fails to record the utter selfishness of the period of the visitation and the dissoluteness and lowered morals which followed in its wake. The surviving laborers insisted on higher wages, and employers used their influence with the Government to pass laws to compel the acceptance of the old rates. Contention raged between rich and poor, and the seeds were sown for Jacqueries and Peasants' Rebellions. The building of churches ceased for a time. The newly laid foundations of the vast nave and choir of the cathedral at Siena were left as they were and have never been built upon to this day. A thousand partially built churches remained stationary for a time, and their construction was resumed only when architectural style had changed so distinctly that the line of division can still be seen. At Oxford and Cambridge and Paris the number of students was depleted and never again rose to its former number. The clergy suffered more than any other class in the community. Many a monastery had lost its whole body of occupants. In others the few survivors, with diminished income from their land and weakened devotion and discipline because of the death of their leading members, never refilled their numbers or regained their old prosperity and vigor. The bishops were compelled to ordain to the service of the church the young, the inexperienced, the illiterate; and even then there were too few for its needs.

Society gradually reconstructed itself on much the same old lines. Permanent changes come not from sudden and conspicuous, but from slowly acting and obscure, influences. Yet Europe was never quite the same again. Two centuries may have passed before the void in medieval population was refilled. Even to this day grain ripens yearly over the sites of villages which lost their population and their name in the great mortality. Various social changes were hastened or retarded or deflected from their former direction. Even though there was no actual breach in the continuity of European development, yet new interests arose, new evil and new good appeared. 'Sithence the pestilence' is an era which Piers Plowman quite naturally uses; for it was graven deep in the memory of his generation.

DISCUSSION AND CORRESPONDENCE.

GEOLOGY AND THE DELUGE.

To the Editor:—I have read with much bewilderment an article entitled 'Geology and the Deluge,' contributed to 'McClure's Magazine' for June, by Dr. Frederick G. Wright, professor of the harmony of science and religion in Oberlin College. Doubtless other men of science would also be gratified if Professor Wright would consent to make his position clear by answering the following questions:

1. You say, Professor Wright, that "The Paleolithic man of science may well be the Antediluvian man of Genesis." Was Tubal Cain, 'an instructor of every artificer in brass and iron,' an antediluvian man, and, if so, had he not learned to use smoothed stone instruments? Was Noah, himself, a paleolithic or a neolithic man, and did he build the ark with flaked or polished flint implements?

2. You say: "But towards the close of this period there were 120 years (specially mentioned in the Bible as a time of warning) in which the movement was accelerated to such a degree that the rising waters gave point to the preaching of Noah." The period of 120 years here mentioned was deduced from the statement that Noah was 600 years old at the time of the flood. Do you believe that Noah was 600 years old, and that his grandchildren that peopled the earth were subseqnently born?

3. You say: "During the last 371 days of this period the catastrophe culminated in the facts specifically related in the Book of Genesis." Do you believe that the 'facts specifically related in the Book of Genesis' are true? For example, that "every living substance was destroyed which was upon the face of the ground, both man, and cattle, and the creeping things, and the fowl of the heaven; and they were destroyed from the earth: and Noah only remained *alive,* and they that *were* with him in the ark."

The readers of 'McClure's Magazine' will probably understand that Professor Wright claims to have confirmed by his geological discoveries the details related in Genesis. Are Professor Wright's fellow geologists also to understand that he believes that the early chapters of Genesis are in accord with modern science and that they are supported by his recent observations in Asia?

X. Plain.

ETHER AND CHLOROFORM.

To the Editor:—In the course of an article on 'Cocaine Analgesia of the Spinal Cord,' in the July number of the Popular Science Monthly, Dr. Jelliffe writes, "Soon after chloroform came ether, the safer anæsthetic," etc. Ether was, however, introduced before chloroform. On September 30, 1846, Morton, of Boston, used ether in dental surgery, and during the following months it was administered in surgical operations at the Massachusetts General Hospital. The news reached England on December 17, 1846. It was used by Simpson, of Edinburgh, in midwifery practice in January, 1847, and it was not until November, 1847, that he announced the discovery of the anæsthetic properties of chloroform.

C. Herrman.

SCIENTIFIC LITERATURE.

RECENT CONTRIBUTIONS TO ENGINEERING.

'TUNNELING: A Practical Treatise,' by Charles Prelini, of Manhattan College, is a well-printed book, just published by D. Van Nostrand Company, which appears to fill a real need, since no American work on the subject has appeared during the past twenty years. The various methods of driving tunnels through earth are fully illustrated, especial attention being given to the shield process which has been so thoroughly developed in recent years. Submarine tunnels are discussed fully, with illustrations of those in New York, Chicago and Milwaukee. Tunnels in rock occupy nearly one-half the volume, the modern methods used in the St. Gothard and Simplon tunnels receiving detailed notice. Subway construction in Boston and New York is also discussed. Interesting historical information regarding ancient tunnels is given, while methods of surveying, centering, blasting and ventilating are explained in a manner which sets forth principles as well as facts. The book is one that shows much painstaking work on the part of the author, and it deserves high commendation.

IN ancient times it was the custom for the title page of a book to give a full account of its contents. This plan is followed in a work by James D. Schuyler, published by Wiley & Sons, whose title is 'Reservoirs for Irrigation, Water Power, and Domestic Water Supply, with an account of various types of dams and the methods and plans of their construction, together with a discussion of the available water supply for irrigation in various sections of arid America; the distribution, application and use of water; the rainfall and run-off, the evaporation from reservoirs, the effect of silt upon reservoirs, etc.' The volume is a large octavo of 400 pages with numerous full-page half-tones and several folding plates. It is mostly devoted to constructions west of the Rocky mountains; here hydraulic-fill dams and rock-fill dams originated, and the book contains descriptions of all that have been built, as well as accounts of the most important masonry and earthen dams. The treatment is descriptive and statistical rather than scientific, and the work is hence mainly one of reference for the use of engineers.

'THE CEMENT INDUSTRY,' published by the 'Engineering Record,' is an octavo volume of 235 pages which gives detailed descriptions of numerous cement plants in Europe and America. The production of natural cement in the United States has been somewhat checked during the last decade by the rapid improvements in the manufacture of the Portland product, particularly by the introduction of rotary kilns. From 1895 to 1900 the production of Portland cement increased from one to seven million barrels per year, and the price suffered a reduction of nearly fifty per cent. The great deposits of argillaceous limestone in the Lehigh Valley form the principal source of Portland cement, but in the west it is made by mixing clay and marl in proper proportions, and there are also two or three plants where blast-furnace slag is used. The book, which is well illustrated, gives full details of the methods of manufacture of both natural and Portland cements.

A MONTHLY journal called 'Revista de Construcciones y Agrimensura' is published monthly at Havana by Aurelio Sandoval. It contains many excellent plans and illustrations of buildings, articles on road and railroad construction, surveying, mechanics and technical education, and appears to be edited with much care. The last number contains the questions propounded to the candidates for chief of the mechanical laboratory in the engineering department of the University of Havana, and these indicate that a high scientific and technical standard is demanded as a qualification for a professorship. The University of Havana was completely reorganized in 1900, under an order issued by Gen. Chaffee, and the previously independent school of engineering made one of its departments under the faculty of letters and sciences. The university recently conferred its first degree of Civil Engineer upon Sr. Andrés Castella, who had the highest rank in an examination held for an assistant professorship in engineering.

'THE ECONOMIC DISPOSAL OF TOWNS' REFUSE,' by W. Francis Goodrich, is one of the 'Engineering Times' Library, published by King & Son, London. The author holds that garbage and street sweepings should be destroyed by fire and in no other way, and he presents facts and figures from all countries showing the great growth of processes of cremation. Dumping street refuse at sea, sorting it out into parts which may be utilized, and the processes of reduction by boiling are summarily dismissed as unworthy of consideration. The volume contains a large amount of information regarding different kinds of crematories, with results of comparative tests, and also a lengthy discussion as to the best kind of chimneys and boilers to utilize the hot waste gases for the purpose of generating power. Cremating furnaces are now in operation at 106 towns in England, there being 18 in London alone, and at 12 towns in Scotland and Ireland. At Bradford, Canterbury, Fleetwood, Oldham and a few other places, the waste gases are utilized for producing electric power for street lighting.

'PUBLIC WATER SUPPLIES,' by Professors F. E. Turneaure and F. H. Russell (John Wiley & Sons), is a comprehensive work covering the entire range of the subject, sanitary as well as constructive. That certain diseases are communicated through water is now thoroughly established, and the demonstration that the water can be rendered harmless by proper filtration is complete. Hence purity as well as quantity is an important factor in the consideration of a modern water supply. The chemical and bacteriological part of such works has heretofore been generally kept aside from the engineering part, but by the cooperation of two authors, both specialists in their respective lines, the difficult task of coordination has been here attempted. The book is mainly designed for engineering students in technical schools, but it cannot be said that the chapters on the chemistry and bacteriology of water are written in such a manner as to produce the best results. The engineering discussions relating to filter beds, walls, reservoirs, mains, standpipes and other details seem, on the other hand, to be clear and complete and likely to be of interest and value to all engaged in planning water supplies. The volume is the largest yet published on this subject in this country, is well printed with the exception of some of the cuts, and contains many carefully prepared descriptions of constructed plants. That the authors have not been completely successful in combining the sanitary and constructive elements is not surprising, in view of the difficulty of the task, but they deserve great credit for their painstaking work and the valuable volume produced.

THE PROGRESS OF SCIENCE.

THE WASHINGTON MEMORIAL INSTITUTION.

It is generally though not universally known that Washington made provision in his will toward the establishment of a national university. For reasons somewhat difficult to understand his bequest has never been used, and only after the lapse of a hundred years has an institution been established in his memory which will fully accomplish under the conditions now existing the great objects he had in view. These have, it is true, in large measure been met by the growth of private and state universities, and by the development of scientific work under the government, but at last these different lines will converge in the Washington Memorial Institution. This has resulted from the union of more or less independent efforts. An enthusiastic group of men and women has long advocated the establishment of a national university, and for this purpose a George Washington Memorial Association was incorporated in 1898. In the same year the Washington Academy of Sciences was organized, giving a definite center for scientific interests in the city and to a certain extent throughout the country. In the same year a committee of the National Educational Association was appointed, which recommended the utilization for research of the scientific and other departments of the government. In the same year a committee of the Association of Agricultural Colleges and Experiment Stations recommended the organization of opportunities for study and research for students of the land grant and other colleges. The year 1898 is consequently an important one in the history of the development of education and research in the United States, and the present year marks the union and fruition of these efforts.

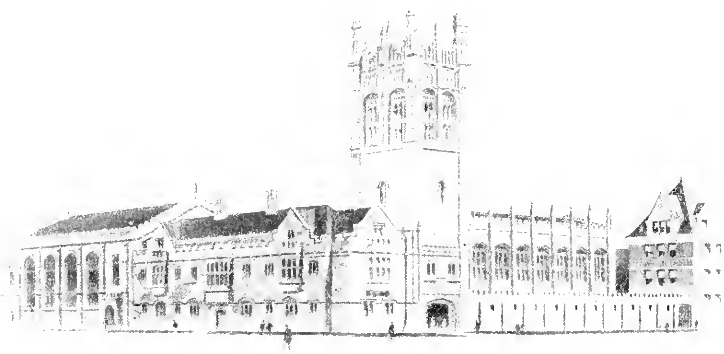
The Washington Memorial Institution was incorporated on May 20, its objects being defined as follows:

> To create a memorial to George Washington, to promote science and literature, to provide opportunities and facilities for higher learning, and to facilitate the utilization of the scientific and other resources of the government for purposes of research and higher education.

A board of fifteen trustees has been created, including representatives of the leading universities of the country and of the scientific work under the government. The officers include Dr. D. C. Gilman, lately president of the Johns Hopkins University, as director; Dr. Charles D. Walcott, president of the Washington Academy of Sciences and director of the U. S. Geological Survey, as president of the board of trustees; and Professor Nicholas Murray Butler, of Columbia University, as secretary of the board. Under these auspices there will surely develop at Washington a great institution, which will in fact be a national university. It will not, however, be a rival to existing universities, but will coordinate their work; it will utilize the opportunities for study and research in the government departments, without interfering with their legitimate functions; it will be a center of research and intellectual activity, worthily representing our great universities and the scientific work of the national government.

THE CELEBRATIONS AT GLASGOW AND CHICAGO.

Glasgow University celebrated its ninth jubilee exactly at the same time that the University of Chicago celebrated its first decennial. On both occasions there were elaborate academic ceremonies, and the sciences were well represented. Indeed at Glasgow science appears to have been predominant, the four principal addresses being in celebration of four men of science, prominently connected with the university in the past. Lord Kelvin delivered an oration on James Watt, Professor Smart on Adam Smith, Professor Young on William Hunter, and Sir Joseph Hooker, in connection with the opening of the new botanical department, on his father. The LL.D. was given to somewhat over one hundred delegates, including, among Americans, J. Mark Baldwin, professor of psychology, Princeton University; William G. Farlow, professor of cryptogamic botany, Harvard University; Adolph Meyer, lecturer on psychiatry, Clark University; and R. Mark Wenley, professor of philosophy, University of Michigan. The celebration at Chicago was even more elaborate, a number of addresses being made on different subjects. The only two foreign delegates appear to have been Professor J. H. van't Hoff, the eminent physical chemist of Berlin, and Professor Marcus Dods, the theologian of Edinburgh. Eleven honorary degrees were given, including in the sciences, in addition to Professor van't Hoff, Professor E. C. Pickering, director of the Harvard College Observatory; Dr. Charles D. Walcott, director of the U. S. Geological Survey, and Professor E. B. Wilson, professor of zoology in Columbia University. The address of most interest from a scientific point of view was Dr. Walcott's on 'The Relation of the National Government to Higher Education and Research.' Mr. and Mrs. Rockefeller were present at the exercises, and President Harper took the occasion to say that the world knows what ten or twelve million dollars will do for a university, but that the time is coming for the world to learn what fifty million dollars can accomplish. As part of the ceremonies the corner stones of a number of new buildings were laid, the most important group of which is shown in the accompanying illustration.



The Leon Mandel Assembly Hall, the Student's Club House, the Tower and the Commons of the University of Chicago.

THE CARNEGIE SCHOOLS.

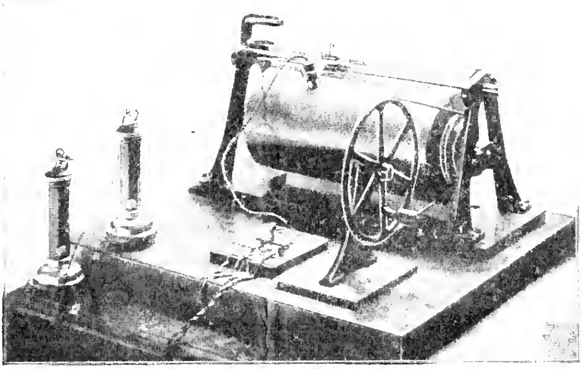
INSTITUTIONS for scientific education and research are developing in the United States with a rapidity that is truly bewildering. Industrial conditions, created by the advance of science, have produced wealth that is both widely distributed and collected in great fortunes. Several of those who have freely received have also freely given. To them the world is forever indebted, for they have not only by their contributions to education and science made new advances inevitable, but, by repaying their debt to society, they have contributed greatly to its stability. During the past month Mr. Jacob S. Rogers, of Paterson, N. J., a manufacturer of locomotives, has bequeathed nearly his entire fortune, $8,000,000 it is said, to the Metropolitan Museum of Art in New York City, and Mr. J. Pierpont Morgan has given $1,000,000 to Harvard University for its Medical School. Mr. Carnegie's gifts of $10,000,000 to the Scottish universities and of an equal sum for American libraries have recently been made, and now it appears that he is planning with equal munificence for technical schools at Pittsburg.

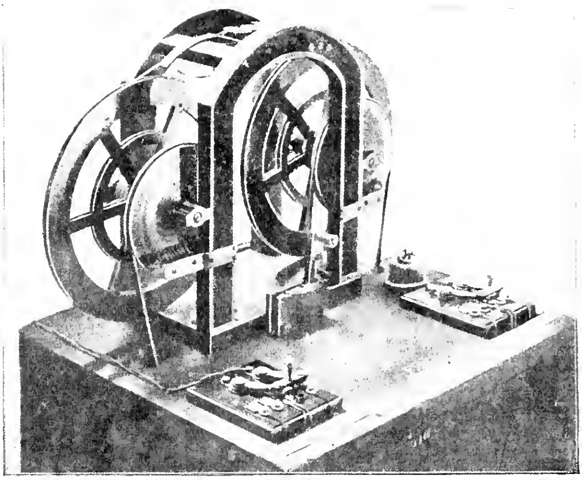
In the May issue of this magazine, Dr. W. J. Holland, director of the Carnegie Museum, described Mr. Carnegie's great foundation. The trustees of the Institute and Library were requested by Mr. Carnegie to draw up plans for technical schools, and they appointed an expert committee, which has just made a report. This committee consists of Professor Robert H. Thurston, director of Sibley Engineering College, Cornell University; Professor J. B. Johnson, dean of the College of Engineering, University of Wisconsin; Professor Thomas Gray, of the Rose Polytechnic Institute, and Professor V. C. Alderson, of the Armour Institute. Their report outlines a technical institute covering the whole field with unparalleled thoroughness, including a college, a high-school and special classes. The college would offer courses in the sciences, in modern languages and in all departments of engineering, and provide the fullest facilities for investigation and research, being in fact a great national school of technology. The high-school would be local in character, but would be a model for the similar schools that will surely be established in other cities. Special classes will provide instruction for those who are unable to give their entire time to study. These are only the recommendations of the committee, but there is every reason to believe that Mr. Carnegie has in view the establishment at Pittsburg of the greatest technical schools in the world.

THE TELEGRAPHONE.

MR. POULSEN, of Copenhagen, has given the name telegraphone to an instrument in which he has most ingeniously combined the telephone and the phonograph. Its general construction will be understood from the illustrations, originally published in the London 'Electrician.' The details of the two instruments differ, a short wire being used in the one and a long steel ribbon in the other, but the general principle is the same. The steel wire or ribbon passes before the poles of an electro-magnet in a telephone circuit, and is thus magnetized in a manner varying with the current in the telephone circuit produced by the voice of the speaker. When the steel wire is then passed over the poles of an electro-magnet, the same undulations will be set up in the current passing through its coils, and the sounds will be reproduced in the receiver. The reproduction is as definite as in a good telephone and much superior in quality to that of any form of phonograph. The record can be used as often as desired, and is said to last indefinitely, but it can be wiped out by passing the wire over an electro-magnet. The wire can be passed over any number of re-

ceiving magnets, and messages can thus be transmitted practically simul-





THE TELEGRAPHONE.

taneously to any number of stations. It is said also that the magnets may, in this way, be used as a telephonic relay, which would be a result of the utmost practical importance.

THE PLANET EROS.

THE little planet Eros bids fair to hold the attention of astronomers for several years to come. Before the observations necessary for the determination of the sun's distance had been completed, came the announcement, by Dr. Oppolzer, of the planet's variability. A variable planet, with a range of variation, such as Eros has shown, is in itself something new and striking, but this is only the beginning of the problem. Several hundred stars are known to vary their light periodically, and some advance has been made in the theory of their variability. Variable stars, however, do not become invariable, neither do invariable stars, after a time, become variable. From a variable planet, having an extremely short period and large range of variation, Eros recently became invariable.

In Europe, soon after the discovery of its variability, its range was said to be two magnitudes, that is, it shone with about six times more light at maximum than at minimum. Precise photometric measurements of the light of Eros, made by Professor Wendell, on March 12 of the present year gave a range of variation of 1.1 magnitudes and on April 12, of 0.4 of a magnitude. On May 6 and 7 no variation was perceptible, and it was less, probably, than a tenth of a magnitude. Owing to poor weather and the planet's approach to the sun, later observations have been difficult. But a slight variation was apparent in June. These unique phenomena probably are the result of the changing direction of the axis of rotation referred to the line of sight. Although the direction of this axis in space is fixed, it will constantly change with reference to an observer on the earth. When the axis, if ever, points directly towards the earth, there can be no variation of light, and the maximum range will be found when the axis is perpendicular to the line of sight. Apparently this axis has recently been pointing towards the earth. We may confidently expect that within a short time Eros will again show well marked changes, although the planet's position may not permit exact observations. On March 5, M. Ch. André communicated to the 'Astron. Nach.' a discussion, in which he assumed that the variation is due to the fact that Eros is a double asteroid. M. André even gave approximate elements for a system which appeared to him to satisfy the conditions. Professor Pickering has recently pointed out that the variations in light can hardly be accounted for by two similar bodies alternately eclipsing each other, and has suggested that the known facts can be explained by the rotation either upon an elongated, cigar-shaped body, or of a body, one side of which is much darker than the other. The solution of the interesting problems, which Eros presents, may not be possible until the next opposition, which does not occur for about two years. Eros will be in conjunction with the sun in the spring of 1902, and in opposition in the summer of 1903. The distance of the planet at that time will be great, since Eros will not be at perihelion, but this will not prevent precise determinations of the changes in light, with a telescope of sufficient power. At the next opposition, however, the path of Eros will be in the southern sky. The most favorable time for observation will be from March to August, 1903. During these months its declination will be between 30° and 45° south of the equator, which will make it difficult or impossible of observation at northern observatories.

JOSEPH LE CONTE.

In the death of Professor Joseph Le Conte, America has lost the man of science who was most honored and beloved. An age of extreme specialization and keen competition can still appreciate the general culture and broad survey of nature which make a great teacher and a great man. Other contemporary men of science have more exact knowledge of a limited field and have made more definite contributions to science, but there is perhaps no one who has done such good work in such diverse directions or whose influence has been so wide and beneficent. Le Conte was descended from a Huguenot family, driven to America after the revocation of the edict of Nantes. His father, uncle and brother were all eminent in science. Born in the South in 1828, he began to practise medicine, but his love of natural science led him to go to Harvard to work under Agassiz. He held chairs in southern universities, but these being disabled by the civil war, he accepted a call to the University of California before its opening, and for thirty-two years has been professor of geology and natural history and the leading scientific man on the Pacific coast. His teaching and

his publications have covered a wide field. His book on 'Sight' is the best English treatise on this subject; he published standard works on geology and recently a work on zoology. His special papers on these subjects and on education and philosophy are numerous and valuable. He was a member of the National Academy of Sciences, president of the American Association for the Advancement of Science and president of the Geological Society of America. He died in the Yosemite Valley on July 6; it seems fitting that death should have come suddenly in the midst of the mountains that he studied and loved. A biographical sketch of Joseph Le Conte, with a portrait, was published in The Popular Science Monthly for January, 1878.

SCIENTIFIC NEWS.

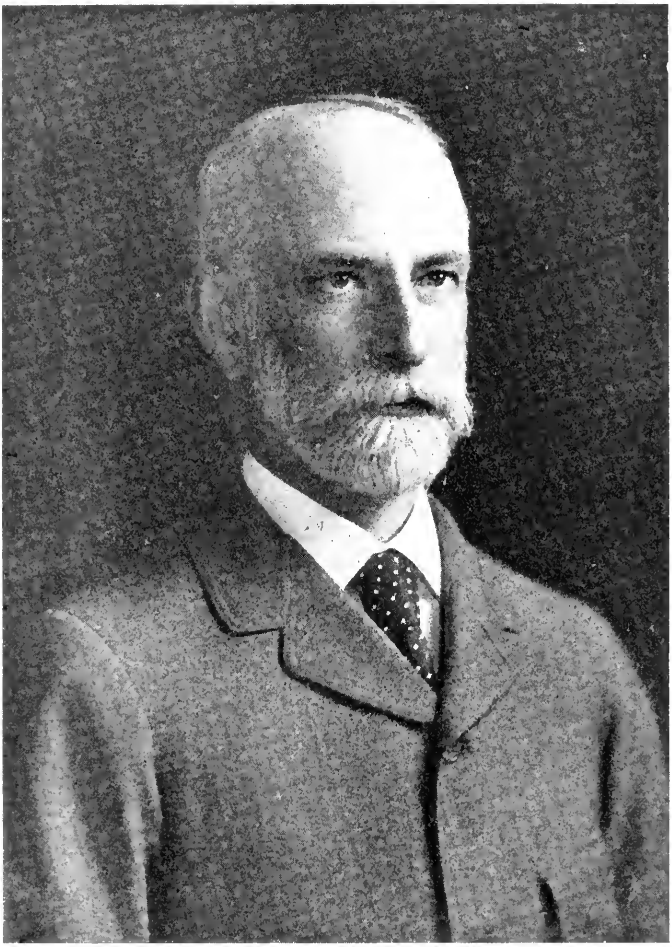
Dr. John Fiske, eminent for his contributions to the theory of evolution and to American history and widely known for his popular writings and lectures, died on July 4. We regret also to record the death of Professor Peter Guthrie Tait, who, for forty years, held the chair of natural philosophy at Edinburgh and made important contributions to physical science.

A committee, consisting of Professors Ira Remsen, J. S. Ames and W. H. Welch, has been appointed at the Johns Hopkins University to arrange a memorial to the late Professor Henry A. Rowland.

M. Laveran, who discovered the malaria parasite, has been elected a member of the Paris Academy of Sciences in the section of medicine, and M. Zeiller a member in the section of botany.—The following fifteen candidates have been elected members of the Royal Society: Professor Alfred William Alcock, Mr. Frank Watson Dyson, Mr. Arthur John Evans, Professor John Walter Gregory, Captain Henry Bradwardine Jackson, Mr. Hector Munro Macdonald, Mr. James Mansergh, Professor Charles James Martin, Major Roland Ross, Professor William Schlich, Professor Arthur Smithells, Mr. Michael Rodgers, Mr. Oldfield Thomas, Mr. William Watson, Mr. William Cecil Dampier Whetham, and Mr. Arthur Smith Woodward.

Professor James Dewar, the eminent chemist, has been elected president of the British Association to follow Professor A. W. Rücker, and will preside at the Belfast meeting in 1902. Professor A. S. Packard, who has held since 1878 the chair of zoology and geology at Brown University, has been elected a foreign member of the Linnean Society of London.

Professor William James, of Harvard University, gave his course of Gifford Lectures on the psychology of religion at Edinburgh during May. —Professor J. H. van't Hoff, of the University of Berlin, gave in June limited number of lectures on physical chemistry at the University of Chicago. —Dr. William Z. Ripley, of the Massachusetts Institute of Technology, has been invited to deliver the second Huxley Memorial Lecture before the Anthropological Institute of Great Britain. The first lecture was given last year by Lord Avebury, and was published in this Journal.



PROFESSOR CHARLES SEDGWICK MINOT,

PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE POPULAR SCIENCE MONTHLY.

SEPTEMBER, 1901.

THE GREATEST BIOLOGICAL STATION IN THE WORLD.*

By Professor W. A. HERDMAN, F.R.S.,
UNIVERSITY COLLEGE, LIVERPOOL.

BIOLOGICAL, Zoological, Marine Stations are all of them merely the seaside workshops of the modern naturalist 'writ large.' But they offer wonderful facilities for the most advanced and best kinds of biological work and it is almost impossible to overestimate the influence they have had in the advancement of our knowledge of living nature. The field-naturalist of old, before the days of college laboratories, studied his animals and plants alive in the open, or collected and arranged them in his cabinets and museums. The work was interesting and necessary, but to some extent superficial. We see its importance enhanced in these later days in the light of Darwinism. It was an enormous gain to science when zoological and botanical laboratories were equipped in the universities, and when every student came to examine everything for himself and to probe as deeply as possible into structure and function. It is no wonder if for a time, in some quarters, in the fascinations of microscopic dissection and section-cutting and mounting, there was perhaps a tendency to lose sight of living nature, and to convert refinement of method and beauty of preparation into the end, in place of being only the means of the investigation.

The biological station came to put all that right. It presented a happy union of the observational work of the field-naturalist with the minute investigations of the laboratory student. It brought the laboratory to the seashore, and the sea, in the form of well-equipped healthy

* From notes taken on a recent visit to the Zoological Station at Naples.

tanks, within the walls of the laboratory. It enabled the living organisms to be studied almost in their native haunts by the most refined laboratory methods.

Thirty years ago the biological station was almost unknown; now there are, I suppose, about fifty or possibly more, large and small, scattered along the shores of the civilized world from the arctic circle to the tropics and Australia, from western California to far Japan in the East—and of these the parent institution, and by far the finest and most important, is the world-renowned 'Stazione Zoologica,' under the direction of Dr. Anton Dohrn, at Naples.

It is almost impossible to think of the Naples station apart from Anton Dohrn. He is the founder, benefactor, director, the center of

THE NAPLES ZOOLOGICAL STATION.

all its activities, the source of its inspiration. He established the first building in 1872, and, although he has had support from the German and Italian Governments and from scientific institutions all over the world, still I believe it is no secret that his own private fortune used unsparingly has contributed much to the permanence and success of the undertaking. He has fostered and directed it continuously for nearly thirty years: the twenty-fifth anniversary of the foundation was celebrated on the 14th of April, 1897, by a remarkable memorial in which all the leading biologists of the world were united.

The international character of the institution is a most interesting and important feature. Situated in the south of Italy, founded and

directed by a German, subsidized (in an excellent manner described below) by most European governments, including even those of Switzerland, Hungary, Holland, Belgium and Spain, the members of the staff and the naturalists at work in the institution may be of any nation and usually are of many; and at any hour of the day at least the four languages, French, German, English and Italian, may be heard among the busy groups in the laboratory and the library.

But the Naples Zoological Station is not wholly for the scientific man—in fact many visitors to Naples do not know that science has anything to do with it. The more public department of the institution, the celebrated 'Acquario,' is one of the sights of Naples and is well known to and highly appreciated by the more intelligent of the tourists you meet at the hotels. The whole institution is usually known to the English-speaking tourist as 'The Aquarium,' and few, even of those who visit and enjoy it, seem to know or wonder anything about the remainder of the great white edifice into the ground floor alone of which they are allowed to penetrate.

The zoological station of Naples in its present condition (it was once smaller, and will probably some day soon be larger) consists of two great, white, flat-topped buildings of imposing appearance, connected by a central yard and large iron galleries, placed on the Chiaja in the Villa Nazionale, the beautiful public garden which occupies that part of the shore of the wonderful Bay of Naples. Surrounded by palms, cactus, aloes, with groups of statuary, fountains and minor temples, looking out upon the incomparable panorama from Vesuvius by Sorrento and Capri to Procida and Ischia, there is probably no finer situation in the world than that occupied by what is unquestionably the most important of zoological institutions.

As to this importance, no university laboratory approaches it. There is no other laboratory where the work-places are occupied by some forty or fifty doctors and professors and investigators of established reputation from all parts of Europe and America, who have come there to do original work, attracted by the fame of the institution and its director; no laboratory where forty such workers can be kept supplied with abundance of fresh material for their researches (of the most diverse description) brought from the sea at least twice a day; no laboratory where there are such excellent facilities for work and such charming opportunities for scientific intercourse.

The staff of the institution now consists of:

1. Professor Dr. Anton Dohrn, the founder and director.

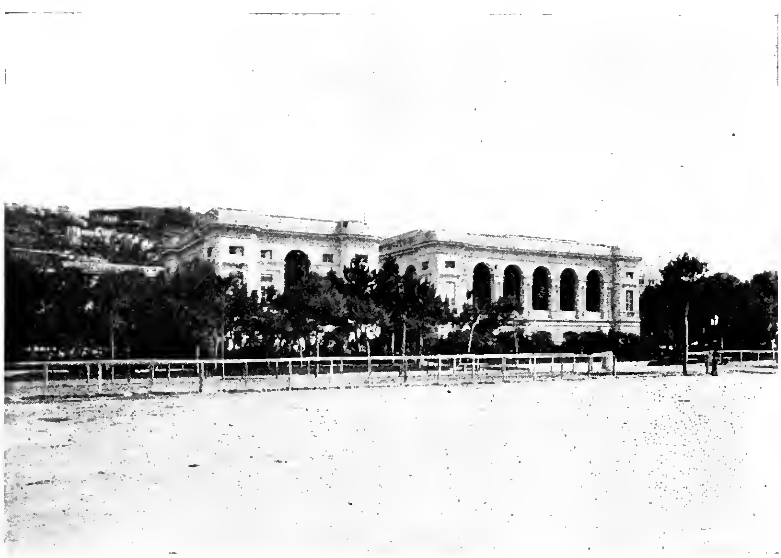
2. Seven Scientific Assistants—viz., Dr. Eisig, the administrator of the laboratories; Dr. Paul Mayer, the editor of the publications, Dr. W. Giesbrecht, the assistant editor, and the supervisor of the illustrations; Dr. Gast, also concerned in the publications in addition to other work; Dr. Schoebel, the librarian; Dr. Lo Bianco, the administrator of the fisheries and préparateur;

Dr. Hollandt, in charge of the microscopic sections department—all of them well-known men, each eminent in his own line of investigation.

The post of assistant in the physiological department formerly held by the late Dr. Schoenlein is now vacant.

3. In addition to these scientific heads of departments there are:—the secretary, Mr. Linden, two painters, and the chief engineer; and, finally, about thirty attendants, collectors and others employed in the laboratories, in the collecting and preserving departments, in the aquarium and elsewhere.

This may seem at the first thought a very large staff, but the activities of the institution are most varied and far-reaching, and everything that is undertaken is carried to a high standard of perfection. Whether it be in the exposition of living animals to the public in the wonderful tanks of the 'Acquario,' in the collection and preparation of choice specimens for museums, in the supply of laboratory material and mounted microscopic objects to universities, in the facilities afforded



THE ZOOLOGICAL STATION FROM THE EDGE OF THE SEA.

for research, or in the educational influence and inspiration which all young workers in the laboratory feel—in each and all of these directions the Naples station has a world-wide renown. And the best proof of this reputation for excellence is seen in the long list of biologists from all civilized countries who year after year obtain material from the station or enroll as workers in the laboratory. Close on 1,200 naturalists have now, since the opening of the zoological station in 1873, occupied work-tables, and, as these men have come from and gone back to practically all the important laboratories of the world, Naples may

fairly claim to have been for the last quarter-century a great international meeting ground of biologists, and so to have exercised a stimulating and coordinating influence upon biological research which it would be difficult to overestimate.

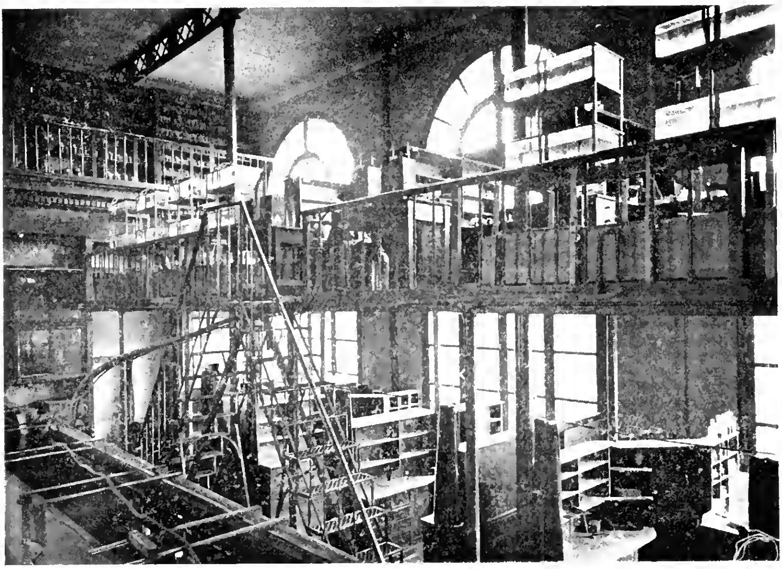
The success of the institution has caused constant additions and has stimulated the staff to fresh undertakings. To the original aquarium and zoological laboratories a second building mainly for botany and physiology and the preparation of specimens was soon added; and it is said that a third is in contemplation. In the meantime additional accommodation has been obtained during the last decade by a rearrangement of the roof of the main building. This gives space for a second large zoological laboratory, a supplementary library and various smaller rooms, used as chemical and physiological laboratories, for photography

LANDING PLACE OF THE FISHERMAN AT POSILIPO, NAPLES.

and for bacteriology. A good deal of the research in recent years, both on the part of those occupying work-tables and of the permanent staff, has been in the direction of comparative physiology, experimental embryology and the bacteriology of sea-water, and all necessary facilities for such work are now provided.

The laboratories contain accommodation for over fifty scientific men to work, and each such work-place, known technically as a 'table,' consists either of a small room or of an alcove or a portion screened off from a larger room. Such tables are rented at £100 a year, not to individuals, but to states or universities or committees, and of the fifty-five tables at present available about thirty-four are permanently engaged—thus bringing in a considerable annual subsidy to the ad-

ministration. Germany takes some ten of these tables, and Italy seven. There are, I believe, three American tables—one belonging to the Women's Association—and there are three English (rented by the Universities of Cambridge and Oxford and the British Association respectively), consequently there are generally about half-a-dozen English and American biologists at work in the station; but Dr. Dohrn interprets in a most liberal spirit the rules as to the occupancy of a table, and, as a matter of fact, during a recent visit of the present writer there were, for a short time, no less than three of us on the books as occupying simultaneously the British Association table, but in reality all provided with separate rooms.

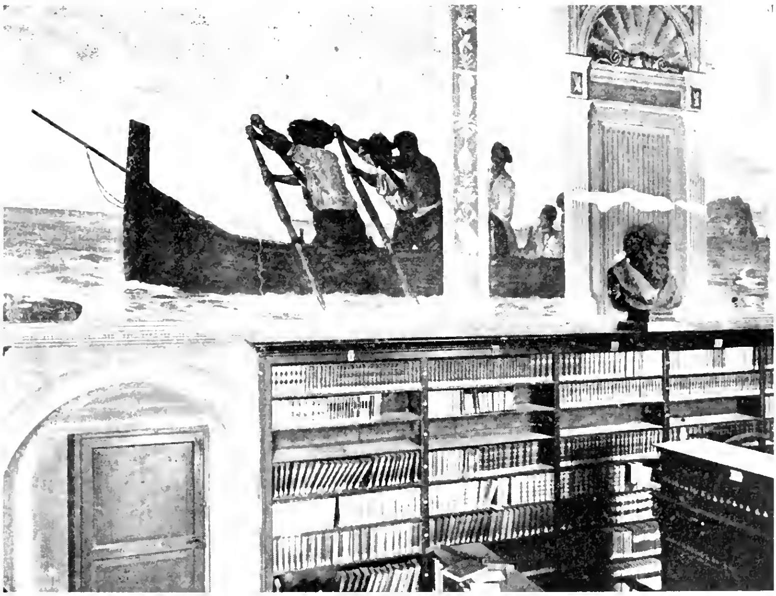


WORK PLACES IN THE LARGE LABORATORY.

A work-table is then really a small laboratory fitted up with all that is necessary for ordinary biological research, and additional apparatus and reagents can be obtained as required. The investigator is supposed to bring his own microscope and dissecting instruments, but is supplied with alcohols, acids, stains and other chemicals, glass dishes and bottles of various kinds and sizes, drawing materials and mounting reagents. Requisition forms are placed beside the worker on which to notify his wishes in regard to material and reagents; he is visited at frequent intervals by members of the scientific staff; he has an attendant to look after his room and help in other ways, and in fact all his reasonable wants are supplied in the most perfect manner. A scientific man, or woman, then, wishing to do a special research at the Naples

station must be appointed to a particular table for a definite time by his government, university, or the controlling committee of that 'table,' and this is the system which has worked so well for over quarter of a century and which gives a certain stamp and tradition to some at least of the tables.

The opportunities for taking part in collecting expeditions at sea are most valuable to the young naturalist, and especially to such as have not had previous experience of the rich Mediterranean fauna. Dredging, 'plankton' collection and fishing are carried on daily in the Bay of Naples by means of the two little steamers (the 'Johannes Müller' and the 'Francis Balfour'—both classic names in biology) belonging to the



In the Library.

station, and by a flotilla of fishing and other smaller boats which start for work in the very early morning and return laden with treasure in time to supply the workers in the laboratory for the day. Many of the Neapolitan fishermen are more or less in the employ of the station and bring to the laboratory such rare specimens as they may chance to find in their day's work.

Chevalier Dr. S. Lo Bianco, the genial chief of the collecting and preserving department, has a phenomenal knowledge of the marine fauna, and of where, when and how to catch any particular thing—and, moreover, of how best to preserve it when caught. Each afternoon he visits the laboratories and ascertains the wants of the workers, each night he

gives his orders to his crews of fishermen, with various hints as to likely haunts and the best tactics to pursue; and the following morning sees a procession of tubs and baskets filled with glass jars, containing the specimens rich and rare, being conveyed from the little dock to the laboratory—generally balanced in wonderful piles on the heads of the stalwart and picturesque boatmen. Dredging expeditions during the day along the shores or to the neighboring bay of Pozzuoli take place in the steam launch, and workers who wish to search for some special animal or who are studying the fauna can join such trips. Then about once a fortnight or so a longer excursion is organized, say to Ischia or to Capri, occupying the whole day, and to this all in the laboratory who care for it are invited. It is on these occasions that Cav. Lo Bianco is seen—if I may say so with all respect—in his glory; directing all proceedings, the center of all activities, full of geniality and information, he is the life and soul of the party. He speaks to us in any language, and knows everything we catch on land or sea; patting the fishermen on the back, talking seriously with the strictly scientific, joking with the more versatile, sympathizing if necessary with the seasick and helping everyone to enjoy the day and profit by the experience, he is an ideal leader of the marine biological picnic.

The finest specimens caught or those not required for immediate investigation are either most skilfully preserved for museums or pass into the tanks of the aquarium. And it is possible, without ever going to sea, to gain a very fair idea of the local Mediterranean fauna from that last named part of the institution. The beauty and interest of the aquarium are due, of course, in great measure to the brilliancy and abundance of the rich fauna in the neighboring waters, but also in part to scientific knowledge and skill. The tanks are most carefully watched and governed, and their exact condition is always known—the temperature, specific gravity, number of bacteria present, and other particulars of the water, are constantly tested and considered. The public admiring the tanks in the ground floor little know of the 'council of war' occasionally summoned in the laboratory upstairs consisting of experts in the subjects concerned, chemistry, biology, bacteriology, to examine some unusual sample or settle some delicate question. And so, by much care and thought, results and effects are produced which we admire greatly in the aquarium and which, although no doubt in part due to the latitude, are also dependent upon the scientific knowledge and manipulative skill behind the scenes.

Amongst the fishes, we see in one tank fine specimens of the Muræna—the real old Roman eel—coiling their snake-like bodies through the necks of broken jars just as their ancestors no doubt did two thousand years ago with the same pots and jars—for those in the tanks are antiques—in the neighboring bay of Baiæ. We can see the Torpedo or

In the Aquarium.

electric ray in an open shallow tank, and by putting the thumb above and the fingers under the animal's flat shoulders, whilst we pull or squeeze the tail with the other hand, an electric shock can be obtained. Octopus, Squids and other Cuttlefish are present in abundance; crabs that mimic their surroundings, those with anemones and with sponges on their backs, animals that look like plants, corals and sea-fans of many kinds, worms that live in leathery tubes a foot long and expand out of the top, like gorgeous flowers six inches across with innumerable spirally-arranged petals—these seem to be the favorites with visitors. But probably the most interesting tanks to the scientific man are those containing the recently caught 'plankton,' the Medusæ and other delicate and gelatinous surface organisms. There is one marvelous creature that can be seen almost nowhere else, the *Cestus veneris,* which is like an undulating, pulsating band of light, in some positions absolutely transparent, in others flashing iridescent fire like a diamond from its sides. So much for the public aquarium, which, at an admission fee of two francs, brings in to the institution a revenue of about £1,000 a year. Now a word as to the publications of the station.

Workers at Naples are free to publish the results of their investigations where they like, and records of the good work in all departments of biology which has been done at this station are to be found in all civilized countries in the form of memoirs and articles contributed to the scientific periodicals of the world. But still a considerable amount of the whole, including a number of the more extended, more solid and more noteworthy contributions, has been published at Naples as a noble series of monographs on the 'Fauna and Flora of the Gulf of Naples'—each monograph being one or more quarto volumes, richly illustrated, and dealing with one particular group of animals, or a section thereof. This great series, of which 26 monographs have now appeared, is amongst the most cherished possessions of every zoological library. Besides these monographs fourteen volumes of a smaller yearly journal, the 'Mittheilungen,' have been published containing shorter but still important papers, and Dr. Paul Mayer also edits a yearly summary or record, the 'Zoologischer Jahresbericht,' of the advances made in all departments of zoology in all parts of the world.

But although the work of the Naples Zoological Station is thus many-sided, the leading idea is certainly original research. An investigator usually goes to Naples to make some particular discovery, and he goes there because he knows he will find material, facilities and environment such as exist nowhere else in the same favorable combination. As a result of the splendid pioneer work which Dr. Dohrn has done at Naples, every civilized country has now established its own biological stations, some larger, some smaller; but although these are of prime importance amongst scientific institutions, as enabling the young

investigator to commence research in living material without leaving home, it must not be thought that they detract from the advantages of a visit to the Naples station, or affect the commanding position of that unique University of Natural History. Notwithstanding Woods Holl, Heligoland and Plymouth—aye, and any others that are likely to follow—Naples is still the Mecca of the young biologist and remains the greatest biological station in the world.

H. Cavendish

HENRY CAVENDISH.

By C. K. EDMUNDS,
JOHNS HOPKINS UNIVERSITY.

PERHAPS the most remarkable character in the history of science is Henry Cavendish. One of the few men of science who have possessed great fortunes, he yet ignored the power of his wealth, allowing himself but few and simple wants. Highest born of the distinguished chemists of Great Britain, he cared nothing for the external advantages of birth, preferring to house himself till forty years of age in his father's stables, where unmolested he might devote his days to the pursuit of truth. Outside the monk's cell and the prisoner's dungeon, few men have lived and held so little communication with their fellows or made so few friendships as he. Yet his fame could not be kept from proclaiming itself even during his lifetime, while to-day he is called the 'Newton of Chemistry' and the 'Father of Quantitative Physics,' being declared by Biot to be 'the richest of scientists, and the most scientific of the rich.'

Of a family, tracing its pedigree to the Lord Chief Justice under Edward III., he was the son of Lord Charles Cavendish, the third son of the Duke of Devonshire, and of Lady Anne Grey, daughter of Henry, Duke of Kent, and was born October 10, 1731, at Nice, Italy, whither his mother had gone on account of ill-health. His mother died two years later, after giving birth to a second son, Frederick; and Cavendish, residing with his father in London till eleven years of age and spending the next eleven years away at school, was deprived at the most critical period of his life of the salutary influences of a happy home, that might have tempered the peculiarities of his character, which in the last analysis, however, are to be referred chiefly to original conformation.

Having entered Peterhouse College, Cambridge, in 1749, he left in 1753 without taking his degree, a step scarcely due to fear of the examinations themselves, but rather in keeping with his very pronounced shyness, which he was said to possess to a degree bordering on disease. His personal history is a blank for the next ten years, but his subsequent writings show that they were spent in mathematical, chemical and physical research. He was elected a fellow of the Royal Society in 1760.

In 1783, the death of his father left him free to follow his own tastes. During his father's lifetime he was kept on an annuity of

£500, and some regard this fact as explaining in part the peculiarities of his character: for during this period he acquired those habits of economy and those singular oddities of character which he ever afterward exhibited in so striking a manner. For some years Cavendish was allowed by his father to attend the Royal Society Club regularly, but was given the exact five shillings for the dinner, not a penny more. There is reason to believe that his father's parsimony has been misapprehended, for while Cuvier, Biot and Lord Brougham make dissatisfaction with Henry for not entering on public or political life the ground for his illiberality towards him, yet others assert that Lord Charles Cavendish was not a rich man and allowed his son all he could afford. There is no certainty as to when or from what source Cavendish inherited the riches which ultimately came into his possession, though they were probably a legacy from a rich uncle. All the testimony, however, is at one on two cardinal facts: that Cavendish was for the first forty years of his life a poor man, and for the last thirty-nine an exceedingly wealthy one.

The possession of several hundred thousand pounds did not alter his life in the least; he simply did not know what to do with it, and hence let it alone, allowing it to accumulate till at his death his estate was worth £1,175,000. Cavendish's indifference to pecuniary affairs was so great that when his banker called on him with regard to the investment of a portion of the vast sum that had grown on his hands, he was rudely ordered to be gone and not to come there to plague him, or he would lose the control of the funds. It may seem strange that none of this large fortune was devoted to scientific or charitable purposes, but we must remember that Cavendish never thought of himself, much less of others. Sir Humphry Davy was indebted to him for 'some bits of platinum,' but tacitly appealed in vain for financial aid in his electrical researches. Just before the subscription for the enlarged voltaic battery was taken, Cavendish was in Davy's apartments at the Royal Institution, and upon Davy expressing fear that he should fail to secure the necessary amount, Cavendish joined heartily in deploring the lack of liberality in the patrons of science, but did not seem to consider himself at all called upon actively to forward the desired object. Yet had he been directly asked to sign a cheque in Sir Humphry's name for £500 he would probably have done so at once. When reminded of some needy charitable object, he gave liberally, but he never himself saw a need. Whether from original or acquired indifference, he exhibited a *passive* selfishness in all his dealings.

To his town residence, close to the British Museum, few visitors were admitted, and these have reported it to contain only books and apparatus. For the former he also set aside a separate mansion on Bradford Square, and here collected a large and carefully chosen library

of science, which he threw open to all engaged in research, and from which he himself never took a book without leaving a formal receipt. His favorite residence was a beautiful suburban villa at Clapham, which now, as well as a street in the neighborhood, bears his name. The whole house was occupied with workshops and laboratory, only a small part being set aside for personal comfort. He needed nothing more for himself, and he did not wish others to visit him. When occasionally he had guests, they were always feasted on a leg of mutton. On one occasion his housekeeper suggested that a leg of mutton would not be enough. Well, then get two, was the reply.

The more prominent of Cavendish's contemporaries have left graphic estimates of his remarkable and interesting peculiarities of character. The most striking was a singular love of being alone. He held aloof from social intercourse, even with members of his own family, and only once a year saw the one he had made his heir. To the great objects of common regard which excite the fancy, the emotions and the higher affections, he was equally indifferent. The beautiful, the sublime and the spiritual seem to have lain entirely beyond his horizon. Although he is thought to have held Unitarian views, he is also understood never to have attended a place of worship. In the words of one of his contemporaries, 'he was the coldest and most indifferent of mortals.' He never married and was reputed to have a positive dislike for women. Lord Brougham tells us that Cavendish ordered his dinner daily by a note, left on the hall table, where the housekeeper could afterwards get it. Another authority relates that Cavendish 'one day met a maid servant on the stairs with a broom and pail, and was so annoyed that he immediately ordered a back staircase to be built.' His dress was that of the gentleman of the preceding half century. The frilled shirtwaist, the greatcoat of a greenish color, and the cocked hat, made a picture no one was likely to mistake. But gleams of genius often broke through this unpromising exterior. He never spoke except to the point, and always supplied excellent information or drew some important conclusion from his own very extensive and accurate knowledge. So that while Sir Humphry Davy said of him, "His voice was squeaking, his manner nervous, he was afraid of strangers, and seemed, when embarrassed, even to articulate with difficulty," he also said, "He was acute, sagacious, and profound, and, I think, the most accomplished British philosopher of his time."

But two additonal dates remain to be given in reference to his personal history: The first, March 25, 1803, when he was elected one of the eight foreign associates of the French Institute; the second, February 24, 1810, when he died, in his seventy-ninth year. As he lived so he died by rule, predicting his death as if it had been the eclipse of some great luminary (as indeed it was), and counting the moment when

the shadow of the unseen world should enshroud him in its darkness. After an illness of only three days, the only one he ever had, he called his servant, told him he was going to die, and commanded him to stay away and to keep everyone else away until the event was over. The servant obeyed, and, when he returned, Cavendish had breathed his last.

An intellectual head thinking, a pair of wonderfully acute eyes observing, and a pair of very skilful hands experimenting and recording, are all that we realize in reading his memorials. His theory of the universe seems to have been that it consisted of a multitude of objects to be weighed, numbered and measured; and the vocation to which he thought himself called was to weigh, number and measure as many of these objects as his threescore years would allow. Whenever we catch sight of him, we find him with his measuring rod and balance, his graduated jar, thermometer, barometer and table of logarithms. Most of his researches were avowedly quantitative; he weighed the earth, he analyzed the air, he discovered the compound nature of water, and he noted with numerical precision the actions of the ancient element fire. Everything pertaining to each, to which a quantitative value could be attached, was set down in figures, before it went out to the scientific world with its passport signed and sealed. In all his researches he displayed the greatest caution, not from hesitation or timidity, but from his recognition of the difficulties which attend the investigation of nature. *Cavendo tutus* was the motto of his family, and seems ever to have been before him, so that he well deserves the title—'Father of Quantitative Physics.'

His first recorded scientific work was 'Experiments on Arsenic' (1764). In 1765 'Experiments in Heat' were performed which, though written out for a friend, were not made public till nineteen years later, but which, had they been published in 1764, would have given Cavendish precedence to Black in some of his discoveries as to 'latent heat' and 'specific heat,' and equal merit in others. In his first public contribution to science, 'Experiments on Factitious Airs,' sent to the Royal Society in 1766, he defines 'factitious air' as air which is driven off when compounds are heated or treated with acids, and the questions of the permanent elasticity of 'factitious airs,' their solubility in different liquids, their power to support combustion, their specific gravity, and likewise their combining equivalents, were all carefully considered. 'Fixed air' (CO_2) was only a particular kind of factitious air driven off from the alkalis (carbonates), and he found that when it was mixed with common air in the proportion of one part to nine, it rendered the air unfit for respiration. Cavendish first isolated and experimented with hydrogen, though he cannot be called its discoverer, for Paracelsus, about 1540, obtained it by acting on metals

with sulphuric acid, the result of which he described as the 'rising of the wind'; and many of Cavendish's predecessors, Boyle among others, had encountered it; but Cavendish, who called the gas 'inflammable air,' was the first to examine its properties carefully and to describe them.* Cavendish is also entitled to be called the discoverer of the constant composition of the atmosphere, and its first accurate analyst, for in 'An Account of a New Eudiometer' (1783) he showed the atmosphere to be of constant composition and to consist chiefly of 'phlogisticated' and 'dephlogisticated air' (nitrogen and oxygen), and he observed that when the electric spark passing through his eudiometer caused the 'phlogisticated' and 'dephlogisticated air' to unite, there was always left a small bubble which he could not get rid of in any way. This small bubble we now know to have been '*argon*.' In his celebrated paper read before the Royal Society in 1784, on 'Experiments on Air,' he gave an account of the discovery of the composition of water and of nitric acid. He showed that nitric acid, which had been known by Geber probably in the eighth century, was produced when nitrogen mixed in small quantity with hydrogen was exploded by the electric spark in the presence of an excess of oxygen. But strictly speaking we cannot assign to him the merit of the discovery of the composition of nitric acid, for he regarded nitric acid as a simple, or at least an undecompounded body, while nitrogen, according to him, was a compound. He was thus not the direct asserter of the modern doctrine of the composition of nitric acid, and to Lavoisier belongs the merit of the true interpretation of Cavendish's results.

Wilson's presentation of 'A Critical Inquiry into the Claims of All the Alleged Authors of the Discovery of the Composition of Water'† makes it certain that Cavendish was the first consciously to convert hydrogen and oxygen into water, and to teach that it consisted of them. In his own words 'water consists of dephlogisticated air united with phlogiston,' and as dephlogisticated air was his term for oxygen and phlogiston his term for hydrogen, this statement corresponds closely with the modern view of the nature of water. His inheritance of the prejudices of the early phlogiston school led him to the erroneous conclusion that every combustible contains hydrogen, and that the deoxidation of air and the oxidation of combustibles are invariably accompanied by the production of water. The discoverer of so great a truth as the composition of water may be forgiven for overestimating its importance.

While his experiments on the composition of water were made in the summer of 1781, his paper, 'Experiments on Air,' was not read

* Lavoisier named the gas 'hydrogen,' i. e., water-former.

† In the 'Life and Works of Cavendish,' by Dr. G. Wilson, published for the Cavendish Society, London, in 1851.

to the Royal Society till January, 1784; and this delay, resulting from his desire to investigate the nature of the acid (nitric) formed on the passage of the electric spark through a mixture of hydrogen and oxygen, containing, as was afterwards found, a little nitrogen, caused his claim to the discovery of the composition of water to be contested by no less rivals than the celebrated James Watt and the great French chemist, Lavoisier. The modest, retiring and almost inordinately cautious man, whose personal history has just been detailed, has been accused of both incapacity and dishonesty, by men distinguished in letters and science, whose connection with the vexed question gives it an interest apart from its intrinsic merit.

Though Cuvier, in 1812, as secretary of the French Academy in reading an *éloge* on Cavendish could say that "his demeanor and the modest tone of his writings procured him the uncommon distinction of never having his repose disturbed either by jealousy or by criticism," Cuvier's distinguished successor, Arago, in writing the éloge on Watt in 1839, charged that Cavendish learned the composition of water, not by experiments of his own, but by obtaining sight of a letter from Watt to Priestley. The French Academy heard the one side argued, and the British Association in the same year heard Cavendish's vindication delivered by the Rev. W. Vernon Harcourt, the president for that year.

At the very threshold of the water controversy we encounter a perplexing dilemma. Two unusually modest and unambitious men, universally respected for their integrity, famous for their discoveries and inventions, and possessed of rare intelligence, are suddenly found standing in a hostile relation to each other, and, although declining to publish their own unquestioned achievements, are seen contending for a single discovery, which the one believes the other to have learned at second hand from the revelations made to a common friend, and which that other accuses his rival of having gathered from a letter that he was allowed to peruse. A misunderstanding such as this would never have occurred had Watt and Cavendish been intimate in 1783. As yet, however, the friendly intercourse which afterwards subsisted between them had not commenced. The one was resident in London, the other in Birmingham, and each was informed of the other's doings by third parties, upon whom mainly though not equally, rests the blame of having occasioned the water controversy. Those in question are: Dr. Priestley, J. A. DeLuc and Sir Charles Blagden, all eminent men of unblemished character. Through the first, knowledge of Cavendish's experiments passed to Watt, and a knowledge of Watt's conclusions to Cavendish; by the second, Watt was informed that Cavendish had deliberately pilfered his theory; and the third, who was Cavendish's assistant, reported the latter's conclusions as well as those of Watt, to Lavoisier, whom he accused of appropriating the ideas of both English

philosophers. Blagden also made certain changes in the manuscript of Cavendish's 'Experiments on Air,' and while superintending, in his capacity of secretary of the Royal Society, the printing of the paper, and of Watt's rival essay, suffered certain typographical errors to occur, which involved himself and his principle in accusations of unfairness, in which, however, Wilson shows that with the exception of a carelessness in correcting printer's proof, Blagden was guiltless of any wrong toward Watt or of unfairness toward Lavoisier, and that to DeLuc belongs the unenviable distinction of deliberately provoking the water controversy, doing Cavendish and also Watt a great wrong by hastily deciding against the former, and filling Watt's mind with suspicions that Cavendish had borrowed from Watt's letter to Priestley the views which he published as his own, because he had in truth discovered them for himself, and that too at an earlier date (1781) than Watt (1784) or Lavoisier (1784-86).

Remarkable as Cavendish's achievements in chemistry were, his greatest fame as a scientist will ever be based on his researches in physics; his single experiment in dynamics would place him in the first rank, and a very large part of the modern development of electrical science is based upon his work in electrostatics and electro-dynamics.

Cavendish verified the law of inverse squares for gravitational attraction, and determined the mean density of the Earth. The apparatus, devised by the Rev. John Michell, consisted of a horizontal lever six feet long, suspended by a wire forty inches in length, carrying at each end a lead ball two inches in diameter. Two large masses of lead (each about 317 lbs.) were placed near the ends of the lever and on opposite sides, so that their attraction would produce turning moments in the same direction. The angle of rotation having been measured by means of telescopes with verniers, and the torsion of the suspending wire determined by observing the time of vibration of the rod, the force was calculated which would have been exerted by a globe of water the size of the earth on the same body on its surface, and from this the density of the earth was obtained as the mean of seventeen terminations to be 5.48 ± 0.38.*

Cavendish cared more for investigation than for publication. He would undertake the most laborious researches in order to clear up a difficulty which none but himself could apprehend, or was even aware of, and we cannot doubt that the result of his enquiries gave him a certain degree of satisfaction. But it did not excite in him the desire to communicate the discovery to others which, in the case of ordinary men of science, generally ensures the publication of their results. How

* This work has recently been edited, together with that of Newton, Bouguer and others, by Professor A. S. Mackenzie in 'The Laws of Gravitation,' Scientific Memoirs, American Book Company.

completely these researches of Cavendish remained unknown to other men of science is shown by the subsequent external history of electricity.

Cavendish's work as a member of the committee appointed by the Royal Society to investigate protection from lightning shows him cooperating with Franklin and others in an investigation on behalf of the nation. But most of his work was a private matter and in electrical science, in which he was by far *the* authority of his day, he published only two papers, 'Of the Electrical Property of the Torpedo' (1776) and 'An Attempt to explain some of the principal Phenomena of Electricity by means of an Elastic Fluid' (1771-72). Yet he left behind him some twenty packets of manuscript on mathematical and experimental electricity, which were but little known till Maxwell edited them in 1879, for they were only alluded to in his celebrated paper on the Torpedo. They anticipated, however, many of the facts subsequently made known by Coulomb and other celebrated physicists, and contained some of the results of experiments of a refined kind instituted at a much later day.

Cavendish proved the law of inverse squares for electric charges not by actually measuring the forces as in the case of gravitational attraction, but by showing that the entire charge resides on the surface of a charged body and that there is no charge at all communicated to a sphere within a sphere when electrically connected and a positive charge is given to the outer one. He then established the theorem that the force must vary inversely as the second power of the distance between charges in order to explain this result of experiment, showing that if it varied according to any higher power, the inner globe would receive a part of the positive charge, if according to any lower power, the inner globe would be negatively charged.

These experiments on the law of inverse squares were performed in December, 1772, and in fact all of his work in electrostatics was completed before 1774, while it was not till 1785 that Coulomb published the first of his seven memoirs, on the data of which the mathematical theory of electricity as we now know it was founded by Poisson; and as Cavendish never published his at all, it is plain that each worked in ignorance of the other's results. The method of each was distinctly his own. Coulomb made direct measurements of the electric force at different distances and compared the density of the surface charge on different parts of conductors. On the other hand, the very idea of the capacity of a conductor as a subject for investigation is due entirely to Cavendish, and nothing equivalent to it occurs in the memoirs of Coulomb. The method that Cavendish adhered to throughout his experimental work was the comparison of capacities, and the formation of a graduated series of condensers, such as is now regarded as the most important apparatus in electrostatic measurements.

Great difficulty is very often encountered in interpreting the work of former experimenters in terms of modern units, yet Cavendish had such a clear insight and worked so quantitatively that we can readily express his results in terms of modern nomenclature and units. His 'inches of electricity,' for instance, can be directly compared with modern measurements, for while his 'inches' express the diameter of a sphere of equal capacity, modern measurements express capacity as the radius of the same sphere in centimeters. When we consider the crudeness of some of Cavendish's apparatus, we are amazed at the accuracy of the results he obtained. The capacity of a circular disc, for example, was determined experimentally by him to be 1/1.57 of that of a sphere of the same radius, while the most modern calculation gives 1/1.571 for the same ratio.

Cavendish also entertained exceedingly clear views on what we now know as 'potential' and 'resistance,' and, besides Coulomb's law of inverse squares, his papers contain anticipations of Faraday's 'specific inductive capacity' and 'electric absorption,' and Ohm's 'law of electrical resistance.' In observing that the charges of coated plates were always several times greater than the charges computed from their thickness and the area of the coatings, Cavendish not only anticipated Faraday's discovery of the specific inductive capacity of different substances, but actually measured its numerical value in some cases. He also considered the question, of fundamental importance in the theory of dielectrics, whether the electric induction is strictly proportional to the electromotive force which produces it, or in other words is the capacity of a condenser the same for high as for low potentials. He regarded his results as not decisive, but, in observing that the apparent capacity of a Florence flask was greater when it continued charged a good while than when it was charged and discharged immediately, Cavendish discovered the phenomenon called by Faraday 'electric absorption,' which was fully studied later for different kinds of glass by Dr. Hopkinson, and connected with the long-known phenomenon of 'residual charge.'

But besides this series of experiments on electric capacity, another course of experiments on electric resistance was going on between 1773 and 1781, the knowledge of which seems never to have been communicated to the world till Maxwell edited Cavendish's electrical researches in 1879. We learn from the manuscripts thus published that Cavendish was his own galvanometer, comparing the intensity of currents by the intensity of the sensations he felt in his wrist and elbows when they passed through his body. The accuracy of his discriminations of the intensity of shocks is truly marvelous, whether we judge by the consistency of his results among themselves or by comparing them with the latest results obtained with a galvanometer, using all

the precautions suggested by experience in measuring the resistance of electrolytes. Indeed, such was the reputation of Cavendish for scientific accuracy, that his bare results were accepted at once and readily became a part of general knowledge, although no one conjectured by what method he had obtained them, more than forty years before the invention of the galvanometer, the only instrument by which any one else has ever been able to compare electrical resistances. In carrying out this work Cavendish not only arrived at the result, which Kohlrausch has since shown to be nearly accurate, that for weak solutions the product of the resistance by the percentage of salt is nearly constant, and also stated accurately the laws of multiple and divided currents, but he even anticipated, in January, 1781, the law of electrical resistance, discovered independently by Ohm and published by him in 1827. Moreover, in a very remarkable set of experiments on a series of salts and acids in order to determine their relative electric resistance, Cavendish tells us, 'that the quantity of acid in each should be equivalent to that in a solution of salt in twenty-nine of water,' and it is difficult to account for agreement not only of the ratios, but also for the absolute numbers given by Cavendish with those of the modern system, in which the equivalent weight of hydrogen is taken as unity. They must have been derived from his own work, for Wenzel's 'Lehre von der Verwandschaften' was published in 1777, and also gives values greater than those used by Cavendish, and Richter's 'Anfangsgrunde der Stochyometrie' was not published till 1792, while Cavendish's experiments were made in 1777. It is only by comparing the dates of these researches with the dates of the principal discoveries in chemistry that we become aware that in the incidental mention of these numbers we have the sole record of one of those secret and solitary researches, the value of which to other men of science Cavendish does not seem to have taken into account after he had satisfied his own mind as to the facts. He dealt with his discoveries as with his great wealth, allowing the larger part of them to lie unused.

A STUDY OF BRITISH GENIUS.

By HAVELOCK ELLIS.

SUMMARY AND CONCLUSIONS.

WE have now examined all those characteristics of the most eminent British persons of intellectual ability which the 'Dictionary of National Biography' enables us to investigate in a fairly generalized manner. We have found that, excluding the living, at least 902 persons (859 men and 43 women) of such preeminent ability have appeared in the British Islands between the fourth and the end of the nineteenth centuries, the century richest in genius being, so far as we can trace, the eighteenth. We have found that, in regard to distribution among the various elements of nationality, England seems to have her fair proportion of eminent persons, Scotland an excess, Ireland and Wales a deficiency, though Ireland and Wales profit considerably among those cases in which there has been intermixture; the only important foreign strain is derived from France. We have found that, as regards social class, the upper and upper middle classes have been peculiarly rich in genius, that the country and small towns have chiefly yielded notable men, and that of all professions the clergy have produced by far the greatest number of distinguished children. Our inquiry, further, confirms the views of Galton and others that intellectual ability is to some extent hereditary, though it may well be that different kinds of ability are not all equally apt to be transmissible. We have found that persons of genius, like the members of other mentally abnormal groups, tend to belong to unusually large families, are much oftener youngest children, and still more eldest children, than in any intermediate position, and that, much more frequently than in the case of the ordinary population, they are the offspring of elderly parents. These eminent persons, we have seen, have in a notable number of instances showed remarkably feeble health during infancy and childhood (being in many cases the only surviving children of large families) but have tended to become more robust as they grew older, and they have been notably precocious. Though not generally subjected to very strenuous intellectual training, they have usually enjoyed excellent opportunities for intellectual development; the majority were at some university; a very large proportion possessed extended opportunities for studying life in foreign lands during youth or early manhood. There is a marked tendency to a celibate life, and marriage when it

occurs tends to take place rather late; there is an excess of sterile marriages, though the fertility of the fertile marriages, while below that of the parents of the eminent persons, does not appear to be small when compared with the general population. We have seen that the longevity of our intellectual persons is great; we have also seen that they show a special liability to suffer from nervous affections like *angina pectoris* and asthma, while gout is peculiarly frequent; insanity is also unusually frequent. Minor mental anomalies, like stammering, are remarkably prevalent. There is also a tendency to melancholy. These are the chief conclusions we have reached concerning British persons of intellectual ability.

It may be reasonable to ask how far these are the characteristics of British persons of genius, and to what degree an investigation of persons of eminent intellectual aptitude belonging to other countries would bring out different results. It is not possible to answer this question quite decisively. The fact, however, that at many points our investigation simply gives precision to characteristics which have been noted as marking genius in various countries seems to indicate that in all probability the characters that constitute genius are fundamentally alike in all countries, though it may well be that minor modifications are associated with national differences. The point is one that can only be decisively settled when similar investigations are carried out concerning similar groups of persons of superior intellectual ability belonging to various countries.

A further question may be asked. How far has confusion been introduced by lumping together persons whose intellectual aptitudes have been shown in very different fields? May not the average biological characteristics of the man of science be the reverse of those of the actor, and those of the divine at the other extreme from those of the lawyer? I believe that Galton is inclined to assume that the investigation of groups of men with different intellectual aptitudes would yield different results. As, however, we have seen, the investigation of eminent British persons, when carried out without reference to the particular fields in which their activity has been exercised, yields results which, when comparable with those of Galton, do not usually show any striking discrepancies. Nor, so far as I have at present looked into the matter, does it appear that on the whole, when we consider separately the various groups of British eminent persons we are here concerned with, such groups show any widely varying biological characters. Certain variations there certainly are; we have seen that the geographical distribution of the various departments of intellectual activity to some extent varies, and also that in pigmentation there are in some cases marked variations. On the whole, however, it would appear that, whatever the field in which it displays itself, the elements that con-

stitute the temperament of genius show a tendency to resemble each other.

I shall probably be asked to define precisely what the 'temperament' is that underlies genius. That, however, is a question which the material before us only enables us to approach very cautiously. There are two distinct tendencies among writers on genius. On the one hand are those who seem to assume that genius is a strictly normal variation. This is the standpoint of Galton.* On the other hand are those, chiefly alienists, who assume that genius is fundamentally a pathological condition and closely allied to insanity. This is the position of Lombroso, who compares genius to a pearl,—so regarding it as a pathological condition, the result of morbid irritation, which by chance has produced a beautiful result,—and who seeks to find the germs of genius among the literary and artistic productions of the inmates of lunatic asylums.

It can scarcely be said that the course of our investigation, uncertain as it may sometimes appear, has led to either of these conclusions. On the one hand, we have found along various lines the marked prevalence of conditions which can scarcely be said to be consonant with a normal degree of health or the normal conditions of vitality; on the other hand, it cannot be said that we have seen any ground to infer that there is any general connection between genius and insanity, or that genius tends to proceed from families in which insanity is prevalent; for while it is certainly true that insanity occurs with remarkable frequency among men of genius, it is very rare to find that periods of intellectual ability are combined with periods of insanity, and it is, moreover, notable that (putting aside senile forms of insanity) the intellectual achievements of those eminent men in whom unquestionable insanity has occurred have rarely been of a very high order. We cannot, therefore, regard genius either as a purely healthy variation occurring within normal limits nor yet as a radically pathological condition, not even as an alternation—a sort of allotropic form—of insanity. We may rather regard it as a very highly sensitive and complexly developed adjustment of the nervous system along special lines, with concomitant tendency to defect along other lines. Its elaborate organization along special lines is built up on a basis even less highly organized than that of the ordinary average man. It is no paradox to say that the real affinity of genius—and I am now speaking of the highest manifestations of human intellect, of genius in so far as it can be distinguished from talent—is with congenital imbecility rather than with insanity. If indeed we consider the matter well we see that it must be so. The organization that is well adapted for adjust-

* In the preface to the second edition of 'Hereditary Genius' Galton has somewhat modified this view.

ment to the ordinary activities of the life it is born into is not prompted to find new adjustments to suit itself. The organic inhibition of ordinary activities is, necessarily, a highly favorable condition for the development of extraordinary abilities, when these are present in a latent condition. Hence it is that so many men of the highest intellectual aptitudes have so often shown the tendency to muscular incoordination and clumsiness which marks idiots, and that even within the intellectual sphere, when straying outside their own province, they have frequently shown a lack of perception which placed them on scarcely so high a level as the man of average intelligence. It is not surprising that by means of the *idiots savants,* the wonderful calculators, the mattoids and 'men of one idea,' and the men whose intellectual originality is strictly confined to one field, we may bridge the gulf that divides idiocy from genius.

Since a basis of organic inaptitude—a condition which in a more marked and unmitigated form we call imbecility—may thus often be traced at the foundation of genius, we must regard it as a more fundamental fact in the constitution of genius than the undue prevalence of insanity, which is merely a state of mental dissolution, in nearly every case temporarily or permanently abolishing the aptitude for intellectual achievement. But it must not, therefore, be hastily concluded that the prevalence of insanity among men of genius is an accidental fact, meaningless or unaccountable. In reality it is a very significant fact. The intense cerebral energy of intellectual creation involves an expenditure of tissue which is not the dissolution of insanity, for waste and repair must here be balanced, but it reveals an instability which may sink into the mere dissolution of insanity, if the balance of waste and repair is lost and the high pressure tension falls out of gear. Insanity is rather a Nemesis of the peculiar intellectual energy of genius exerted at a prolonged high tension than an essential element in the foundation of genius. But a germinal nervous instability, such as to the ordinary mind simulates some form of insanity, is certainly present from the first in many cases of genius and is certainly of immense value in creating the visions or stimulating the productiveness of men of genius. We have seen how significant a gouty inheritance seems to be. A typical example of this in recent years was presented by William Morris, a man of very original genius, of great physical vigor and strength, of immense capacity for work, who was at the same time abnormally restless, very irritable, and liable to random explosions of nervous energy. Morris inherited from his mother's side a peculiarly strong and solid constitution; on his father's side he inherited a neurotic and gouty strain. It is evident that, given the robust constitution, the germinal instability furnished by such a morbid element as this—falling far short of insanity—acts as a precious fermentative ele-

ment, an essential constituent in the man's genius. The mistake usually made is to exaggerate the insane character of such a fermentative element, and at the same time to ignore the element of sane and robust vigor which is equally essential to any high degree of genius. We may perhaps accept the ancient dictum of Aristotle as reported by Seneca: 'No great genius without some mixture of insanity.' But we have to remember that the 'insanity' is not more than a mixture, and it must be a finely tempered mixture.

This conclusion, suggested by our survey of British persons of preeminent intellectual aptitude, is thus by no means either novel or modern. It is that of most cautious and sagacious inquirers. The same position was, rather vaguely, adopted by Moreau (de Tours) in his *Psychologie morbide dans ses rapports*, etc., published in 1859, though, as his book was prolix and badly written, his proposition has often been misunderstood. He regarded genius as a 'neurosis,' but he looked upon such 'névrose' as simply "the synonym of exaltation (I do not say trouble or perturbation) of the intellectual faculties, . . . The word 'neurosis' would indicate a particular disposition of the faculties, a disposition still in part physiological, but overflowing these physiological limits"; and he presents a genealogical tree with genius, insanity, crime, etc., among its branches; the common root being 'the hereditary idiosyncratic nervous state.' J. Grasset, again, more recently (*La supériorité intellectuelle et la névrose*, 1900), while not regarding genius as a neurosis, considers that it is united to the neuroses by a common trunk, this trunk being a temperament and not a disease. The slight admixture of morbidity penetrating an otherwise healthy constitution, such as the present investigation suggests as of frequent occurrence in genius, results in an organization marked by what Moreau calls a 'neurosis' and Grasset a 'temperament.'

It has been necessary to state, as clearly as may be possible, the conclusions suggested by the present study as regards the pathological relationships of genius, because, although those conclusions are not essentially novel, the question is one that is apt to call out extravagant answers in one direction or another. The most fruitful part of our investigation seems, however, to lie not in the aid it may give towards the exact definition of genius—for which our knowledge is not sufficient—but in the promising fields it seems to open out for the analysis of genius along definite and precise lines. The time has gone by for the vague and general discussion of genius. We are likely to learn much more about its causation and nature by following out a number of detailed lines of inquiry on a carefully objective basis. Such an inquiry, as we have seen, is difficult on account of the defective nature of the material and the lack of adequate normal standards of comparison. Yet even with these limita-

tions it has not been wholly unprofitable. It has enabled us to trace a number of conditions which, even if they cannot always be described as factors of the genius constitution, clearly appear among the influences highly favorable to its development. Such a condition seems to be the great reproductive activity of the parents, the child destined to attain intellectual eminence in many cases alone surviving. The fact of being either the youngest or the eldest child is a condition favorable for subsequent intellectual eminence; and I may add that I could refer to numerous recent instances of large families, in which the eldest and the youngest, but no other members, have attained intellectual distinction. We have further seen that there is a tendency for children who develop genius to be of feeble health, or otherwise disabled, during the period of physical development. It is easy to see the significance of this influence which by its unfavorable effects on the development of the limbs—an effect not exerted on the head which may thus remain relatively large—leaves an unusual surplus of energy to be used in other directions; at the same time the child, who is thus deprived of the ordinary occupations of childhood, is thrown back on to more solitary and more intellectual pursuits. The clumsiness and other muscular incoordinations which we have found to be prevalent—while there is good reason to believe that they are of congenital origin—cooperate to the same end. Again, it is easy to see how the shock of contact with a strange and novel environment, which we have proved to be so frequent, acts as a most powerful stimulant to the nascent intellectual aptitudes. It is possible to take a number of other common peculiarities in the course of the development of genius and to show how they either serve to inhibit the growth of genius along unfruitful lines or to further it along fruitful lines.

Such an investigation as the present is far from enabling us to state definitely all the determining factors of genius, or even all the conditions required for its development. It suggests that they are really very numerous and that genius is the happy result of a combination of many concomitant circumstances, though some of the prenatal group of circumstances must remain largely outside our ken. We are entitled to believe that the factors of genius include the nature of the various stocks meeting together in the individual and the manner of their combination, the avocations of the parents, the circumstances attending conception, pregnancy and birth, the early environment and all the manifold influences to which the child is subjected from infancy to youth. The precise weight and value of these manifold circumstances in the production of genius it must be left to later investigators to determine.

THE STATISTICAL STUDY OF EVOLUTION.

BY PROFESSOR C. B. DAVENPORT,

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AS I was going through the chemical section of the John Crerar Library the other day I stopped to examine two books. The first was one of those dawning-chemical works—Glauber's—which enjoyed a wide reputation in the sixteenth and seventeenth centuries. It contained an abundance of speculation and of recipes; but the recipes were of a purely qualitative sort—mix this and this, the *amount* is immaterial. The second book was the lectures of Van't Hoff, marking the most recent development of the science of chemistry. This book is full of formulae and tables; numbers, signs and quantities fill every page; they are symbols not only of quantitative relations but also of the direction and cause of advance of chemistry since the days of the alchemists. The history of chemistry is the history of the other quantitative natural sciences, of astronomy and physics. As science advances its methods become more and more quantitative.

Biology is often contrasted with physics and chemistry as a qualitative science. But there is nothing so fundamentally dissimilar in the phenomena of chemistry and biology that they must necessarily be studied so differently. Both treat of matter, not only statically but also dynamically. But the phenomena of biology are more complex than those of chemistry, the things to be described and compared are more numerous; and so the science, which is hardly more than a century old, is still in the descriptive and comparative stage. But the history of science in general justifies the prediction that biology, too, will in time use chiefly quantitative methods in studying processes.

Evolution is an organic process. It has been studied in various ways. Many have sought by ingenious logic to discover its workings. Others have reasoned by analogy from the effects of artificial breeding. Others still, having observed a probable factor of evolution in one case, have argued for its universality. But all of these methods have been qualitative. More recently, on the other hand, there has been undertaken an exact, quantitative study of the condition of species in nature under different environments, to determine exactly the effects that the different environments have produced. This new method, which now demands our attention, is nothing less than the quantitative study of evolution.

We all know that a lot of people, taken at random, show individual physical differences. Even when the people form a homogeneous lot and are of one sex, are all adults, and are of one race, and when such individuals as are very pathological or freaks are excluded, we still find that they differ in stature, weight, proportions of trunk and appendages, color of hair and eyes, proportions of facial features and various other characters. These differences are so slight and multitudinous that the language of adjectives fails to indicate them; measurements must be made in order that they may be expressed numerically.

But how can these numbers tell us anything about the evolution of the human species? The general principle is this: The differences between the races of man are of the same kind as, and differ only in degree from the differences between the individuals of a race. Consequently, the laws of individual variation in a race may be relied on to illuminate the method of origin of the race or species under consideration. Just how, will be clearer after we have considered the sorts of individual variation.

The laws of nature are got at only with the key of the proper method. And it is only within recent years that an adequate quantitative method has been developed in biology. It will now be necessary to consider this method.

Imagine a file of 40 men arranged in order of stature—the tallest at the head of the column. The crown of their heads will form a flowing curve, nearly level in the middle of its course and becoming more oblique at the ends. The reason for this is that the middle statures are much more common than the extreme ones. Half the people have a stature within two inches of the average (Fig. 1). The general features of such a curve are common to all classes of men, but the details differ in different classes of men. The characteristic differences are measured by what are called the *constants* of the curve.

The stature of the middle man in the file will give us very nearly the first constant, the mean or average. The average is obtained exactly by adding all the individual statures together and dividing by the number of men (*e. g.*, by 40). The average is used constantly and as a matter of course by nearly every one who wishes to compare two comparable series of numbers. It is awkward to compare all the separate data so we let the average stand for the lot. The average is, however, an entirely ideal quantity which need not agree with the measurement of any individual; and it is a little curious that it is so universally used in statistics. Of a series of measurements made on one and the same dimension, the average is demonstrably nearest the true value, and consequently engineers, physicists, astronomers and others who aim at the greatest possible precision in the measurement of the individual

phenomenon must make use of it. But in measurements made on a series of distinct individuals the average does not signify the value nearest the truth, and we cannot infer that it is the most significant single representative of the series. The average has this disadvantage, moreover, that the introduction of a few very extraordinary individuals has undue influence on the result. Thus in calculating the average income of American colleges, one institution with an income of $1,200,000 increases the average by an amount ($2,500) equal to the total income of about 5 per cent. of the 'colleges and universities.' The average has indeed been over-rated and over-used, as though it were always the best single representative of a series; whereas there are other representatives which are sometimes superior. Among these is the *middle value*, which is usually got without much calculation. It

FIG. 1. FILE OF FORTY UNIVERSITY OF CHICAGO STUDENTS ARRANGED (APPROXIMATELY) IN ORDER OF HEIGHT.

is the value above and below which fifty per cent. of the cases lie. In the case of income of American colleges the middle value is not far from $15,000, while the average is $43,000; the former amount unfortunately gives the truer idea of the usual American college; for about 80 per cent. of the colleges have an income of less than $43,000 Still another representative value is the geometric mean which is especially important in many biologic and economic statistics. The geometric mean is the number corresponding to the *average of the logarithms* of the individual quantities.

Finally, if there is one representative of a biological series that is more apt to be significant than any other, it is the value that occurs with the greatest frequency or, in other words, the commonest value. Since this value may be said figuratively to be the most fashionable one,

it has been called the *mode*. The peculiar value of the mode lies in this, that it is not the result of calculation and is not an ideal value merely, but is the prevailing or typical actual condition. In biological statistics the mode should always be considered.

No single value can, however, adequately take the place of all the values obtained. Nevertheless, it is necessary to combine these data in some unit for purposes of comparison. The best unit is the so-called 'frequency polygon.' The frequency polygon is got first by sorting out the data into a number of equally extensive 'classes'; then by laying off these classes as a series of points at equal intervals of space along a horizontal base-line; by erecting perpendiculars proportional to the frequency of each class, and by joining with a line the tops of all these perpendiculars. Or, if the tops be united by a flowing line, the frequency polygon is replaced by the frequency curve.

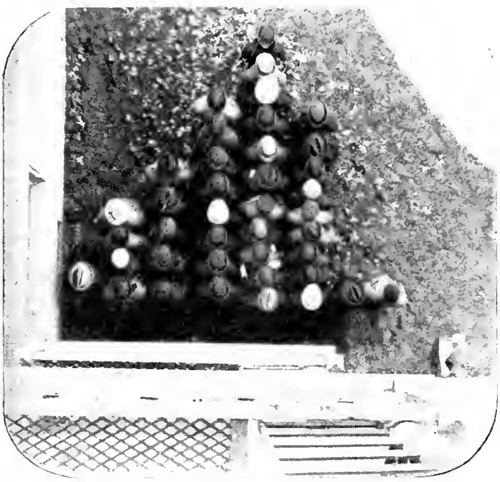

FIG. 2. BIRD'S-EYE VIEW OF 40 UNIVERSITY OF CHICAGO STUDENTS ARRANGED IN FILES BY CLASSES OF STATURE.

Such frequency polygons may also be obtained, without drawing on paper, by putting the individuals belonging to the same class in the same vertical column and arranging the columns in order along a common base-line. For example, we may separate our university students into stature classes as follows: 56 to 57.9 inches, 58 to 59.9, 60 to 61.9; 62 to 63.9; 64 to 65.9; 66 to 67.9; 68 to 69.9; 70 to 71.9; place those falling into the same stature class in a file; and place the files next each other in order, all starting from a common base-line. Then if we take a bird's-eye view of this body of students, we get the frequency polygon of their statures (Fig. 2). The construction of frequency polygons may be illustrated by another example. The common scallop shells of the Atlantic coast have a variable number of

'ribs' (Fig. 3). In any hundred individuals from one locality the number of ribs may vary from 15 to 21. If we put in one pile the shells having the same number of ribs and arrange the piles in order, from 15 to 20, upon a level base we shall get a figure which is the frequency polygon for the ribs of Pecten shells from the given locality (Fig. 4).

Frequency polygons may be obtained in the same way from measurements or countings made on almost any organ of any plant or animal. The shapes of the polygons are probably never exactly alike in different organs; consequently, there is a field for the comparing of polygons and for drawing interpretations from differences in their form.

In comparing frequency polygons attention should be directed, first of all, to two characters; namely, the position and relative proportion of individuals included in the modal class and the spread of the polygon at the base. This spread is known technically as the *range*. While

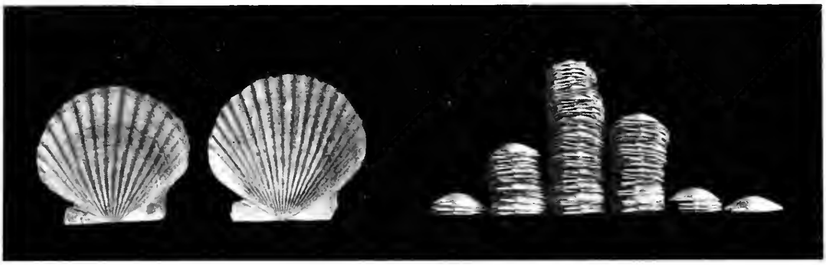

FIG. 3. SCALLOP SHELL WITH 15 AND 20 RIBS RESPECTIVELY.

FIG. 4. SELF-FORMED FREQUENCY POLYGON OF PECTEN RIBS.

some frequency polygons have a high mode and narrow range others have a low mode and a broad range. The importance of this fact is that a narrow range implies relatively small variability, since relatively few individuals depart far from the modal condition. On the other hand, wide range implies great variability. Range, however, is not an accurate measure of variability because it is too easily affected by the accidental occurrence of even one aberrant individual. We need a measure of variability that shall take into account the departures of *all* the individuals from the mode. One such measure is the arithmetical average of all the departures from the mean in both directions; and this measure has been widely employed. At present another method is preferred; namely, the square root of the average of the *squared* departures. This measure is called the standard deviation. The standard deviation is of great importance, because it is the index of variability. This index in the case of measured organs is, like the range, a concrete number; consequently indices are not always comparable, being expressed, *e. g.*, in feet, millimeters, degrees or pounds. So it has been proposed to reduce all indices (except those based on countings) to

percentages of the average value. This proportional index is called the coefficient of variability. So much then for the principal operations: counting or measuring; seriation of the numbers; determination of the mode, average, standard deviation, and coefficient of variability.

The results of comparative variation studies, so far, fall into two

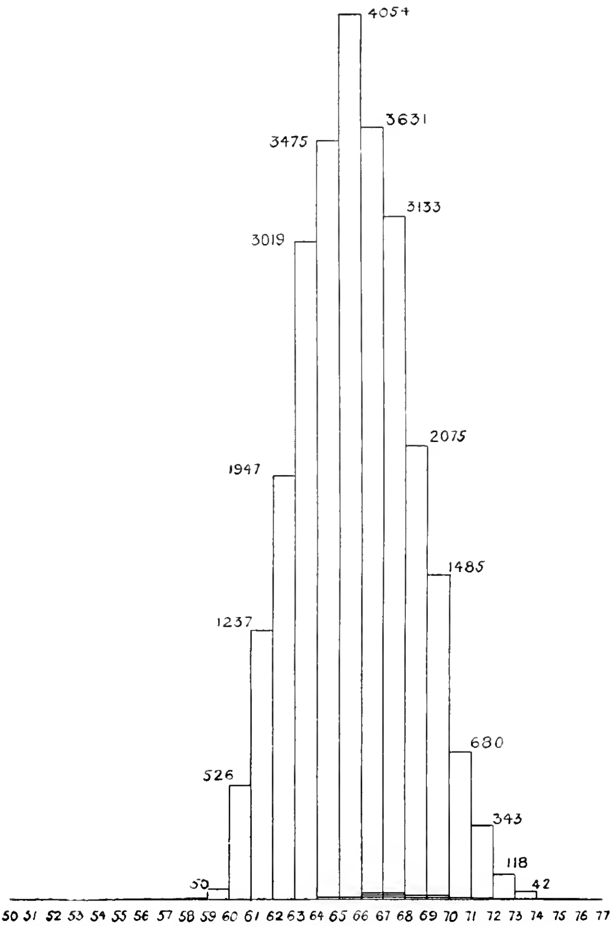

FIG. 5. FREQUENCY POLYGON OF STATURES OF 25,820 AMERICAN MALES; LOWER POLYGON, OF 100 UNIVERSITY STUDENTS.

general categories. First, we have got some notion of the classes of variation polygons that may occur among organisms; secondly, we have some evidence as to the significance of these different forms. As the science is only about five years old it is but to be expected that a satisfactory solution of even the principal problems concerning polygon

interpretation is still to be worked out. The beginnings are, however, instructive.

Variation polygons are of two main sorts—simple and complex. Simple variation polygons possess only a single mode, whilst complex

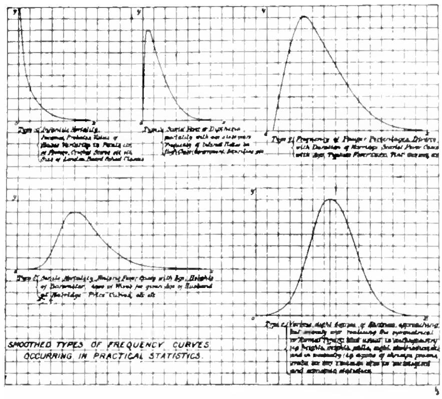

FIG. 6. TYPES OF FREQUENCY CURVES. FROM K. PEARSON.

polygons usually show a trace of more than one mode. Simple variation polygons are of several types. Some are symmetrical about the mode as in Fig. 5. Others are more or less unsymmetrical or skew, as

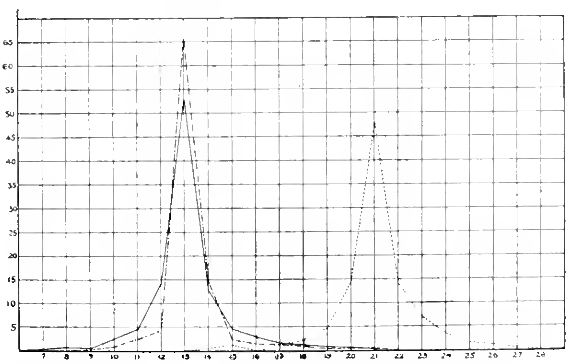

FIG. 7. CURVES OF UNSELECTED AND SELECTED LOTS OF WHITE DAISIES.
——— CURVES OF FREQUENCIES OF VARIOUS MEMBERS OF RAY FLOWERS IN WILD DAISIES.
— - — " " " " " " " IN DESCENDANTS OF 12 OR 13-RAYED WILD DAISIES.
- - - - " " " " " OF RAY FLOWERS IN DESCENDANTS OF 21-RAYED WILD DAISIES.

they are technically called (Fig. 6). Skew polygons usually tail out at the base further from the mode on one side than on the other. The polygon is said to be skew in the direction of this longer partial base. Why some distributions are symmetrical and others skew is not fully

understood, but at present it seems probable that we get symmetrical polygons when the organ measured is not undergoing evolution and, on the other hand, that unsymmetrical polygons indicate evolutionary progress. Also the direction of skewness is probably determined by the direction of evolution. At present, however, we can only say that in many cases the skew polygon tails off (or is skew) in the direction from which the race is evolving. This conclusion, which I believe to be new, is based upon certain results of experiments as well as upon data gathered from material which had developed under natural conditions. Of this material the most important for our purpose is that in which two polygons have apparently arisen by a splitting off from the original polygon of the two extremes which now form two distinct and widely separated types. The first case (Fig. 7) is derived from the common

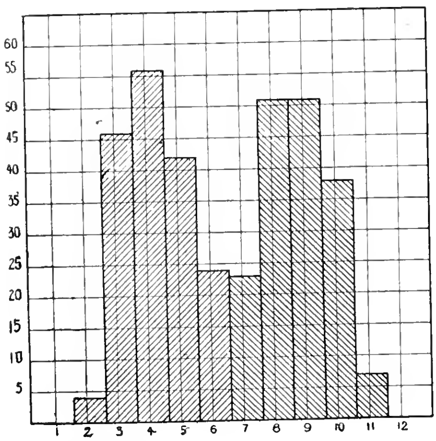

Fig. 8. Polygon of Frequencies of Lengths in Millimeters of Cephalic Bones of 343 Rhinoceros Beetles. From Data of Bateson.

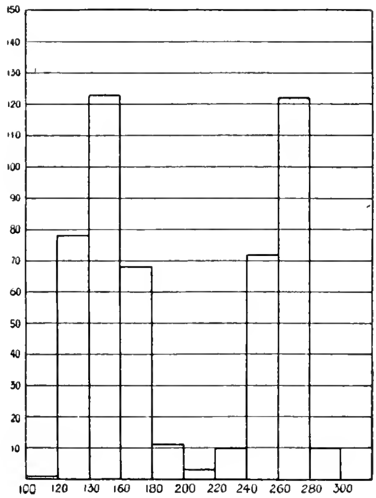

Fig. 9. Polygon of Frequency of Lengths of Wing, in Hundredths of a Millimeter, for a Lot of Chinch Bugs.

white daisy. In the figure the full-line polygon gives the frequency distribution of the ray-flowers in a collection of wild daisies. This polygon has a + skewness of 1.18. The left-hand, dot-and-dash, polygon gives the ray frequences in the descendants of 12- or 13-rayed wild plants. The positive skewness is increased as a result of this selection to + 1.92. The right-hand polygon gives the ray frequencies in the descendants of the 21-rayed plants, a single highly aberrant case of 32 rays being omitted. The skewness is — 0.13. In this case we have experimental evidence that curves are skew *towards* the original, ancestral, condition. The cases in which the frequency curve is bimodal frequently signify that two races are arising out of a former

ancestral intermediate condition; *i. e.*, they correspond to the right- and left-hand polygons of Fig. 7. Consequently we may expect them to be skew in opposite directions and so we find them to be. For example, Bateson has measured the horns on the heads of 343 rhinoceros beetles; the frequency curve is shown in Fig. 8. The left-hand polygon has a skewness of + 0.48; the right-hand polygon of — 0.03. One might infer that the right-hand form, the long-horned beetles, had diverged less from the ancestral condition than the short-horned beetles. Again, my pupil, Mr. Garber, has obtained a bimodal distribution polygon in the length of the chinch bug's wing (Fig. 9). The

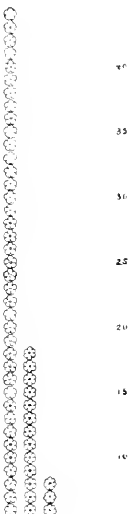

FIG. 10. FREQUENCY POLYGON OF THE NUMBER OF PETALS IN BUTTERCUPS.

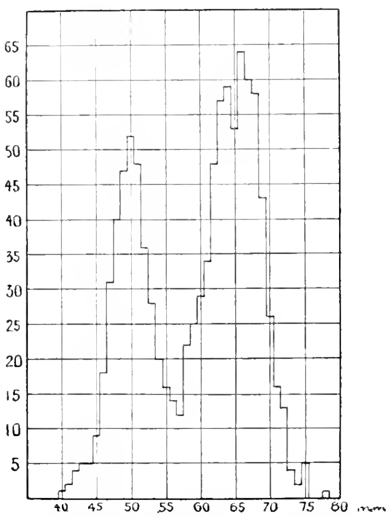

FIG. 11. POLYGON OF FREQUENCY OF LENGTHS IN MILLIMETERS, OF PECTEN SHELLS GATHERED AT RANDOM FROM A FISHERMAN'S SHELL HEAP.

short-winged form has a skewness of + .44; the long winged form of — .43. In this case also the ancestral form lies between the present modes. It is obvious that we may get cases in which two modes, representing conditions in different places, have moved, to different extents, in the same direction. Thus the index (breadth ÷ length) of the shell of Littorina, a marine snail, as measured by Bumpus, has at Newport a mode of 90; at Casco Bay of 93. The skewness is positive in both places and greater (+ .24) at the more southern point than at Casco Bay (+ .13). This result indicates that the Littorina came from a more northern home, for which we have confirmatory historical evidence, and that these ancestral races were rounder, having an index

possibly not far from 96. Likewise the Littorinas from South Kincardineshire, Scotland, have a modal index of 88 and a positive skewness of 0.065; while those of the Humber, having a mode of 91, have a skewness of + 0.048. These figures suggest an ancestral index of about 97, or about the same as before. The form of the frequency polygon may thus enable us to infer the ancestral condition of a race or species and may consequently help us to get at the history of the race.

Skewness may, as we have seen, depart to any extent from a nearly symmetrical condition. The extreme case occurs when the mode lies at one end of the range. This case is sometimes found among plants. It indicates that the group has in respect to the character in question reached an extreme condition (Fig. 10).

Complex frequency polygons have various interpretations. We have already seen that one of these interpretations at least is a splitting of one race into two. Another kind of complex polygon is due to

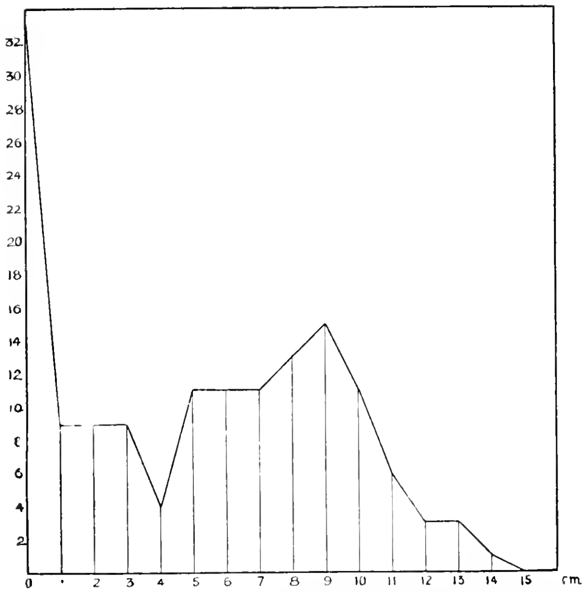

FIG. 12. COMPLEX CURVE OF LENGTH IN CM. OF FASCIATED STEM OF 146 INDIVIDUALS OF CREPIS BIENNIS DERIVED FROM FASCIATED ANCESTORS. ONE INDIVIDUAL OF 19 CENTIMETERS OMITTED. FROM DE VRIES, '95, BULLETIN SCIENTIFIQUE, TOME 27, P. 397. ORDINATES, NUMBER OF INDIVIDUALS; ABSCISSÆ, LENGTH OF FASCIATED STEM.

differences of age. Suppose an animal that breeds at one restricted season of the year and that annually nearly doubles in size. If we make a collection of a lot of these animals from a place at one time, we shall include individuals of different ages such as, for instance, six months, one year and six months, two years and six months, and so

on. If now the length of all these individuals be measured we shall obtain a series of modes of which each corresponds to one of the broods (Fig. 11). Still again, two modes may appear when the material is not perfectly homogeneous although the age be constant. For instance the material may contain both normal and abnormal individuals. An example of this sort of polygon is given in Fig. 12. A very complex curve is afforded by the number of the ray flowers of composite plants. If the lappets of a thousand white daisies be counted it will be found that there is not a single mode only but a series of them. These modes increase in height from one extremity of the range, reach a great mode at one point and then diminish again (Fig. 13). It appears also that the modes do not occur at haphazard,

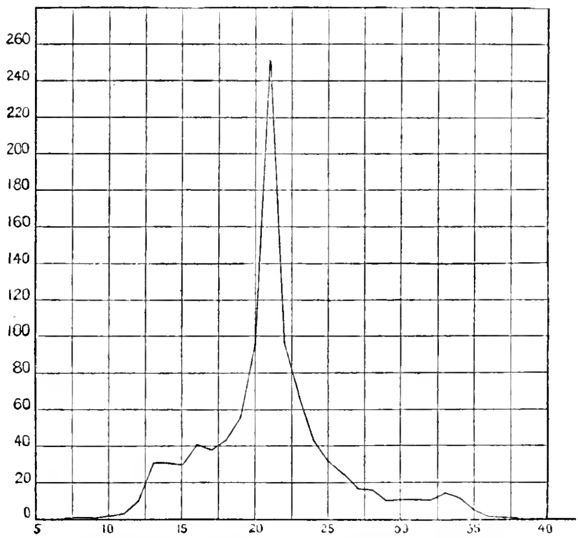

FIG. 13. POLYGON OF FREQUENCY OF NUMBERS OF RAY FLOWERS OF THE WHITE DAISY GATHERED AT RANDOM FROM VARIOUS GERMAN LOCALITIES. FROM DATA OF LUDWIG.

but chiefly in the series of numbers: 1, 2, 3, 5, 8, 13, 21, 44, and 65. This is a mathematical series in which each term is the sum of the two preceding. Also the ratios of these numbers, namely, ½, ⅓, ⅖, ⅜, and so on, have long been known to represent the arrangement of leaves on a stem; and this seems to be why the numbers of this series are so prominent in the rays of the flower head.

The comparative study of frequency polygons, such as we have been making, enables us, it will be seen, to distinguish different kinds of variation and to make that philosophical classification which is the first step in advancing knowledge. Although the causes of variation are not at once revealed, we are directed to working hypotheses that

can be tested by experiment. It is not too much to say that the frequency polygon is the key to the first door that has barred true progress in the difficult subject of the origin of organic diversity.

In what has gone before we have considered variation of single organs or qualities of a species. Yet, although we have to study the variation of organs taken one at a time, in nature no organ undergoes variation by itself alone. For the parts of the body are so knit together, their morphological kinship or their physiological interdependence is such, that when one organ deviates from the mode many others deviate also. This fact has long been known as *correlation* of variation. A recognition of the law by Cuvier was the justification, slight though it was, of his premature attempts to reconstruct an extinct form from one of its bones. Now, correlation is of great importance in the origin of species; it makes it easier to understand how evolution can take place. For example, when it was objected that natural selection by acting on one part at a time could hardly build up so complex a structure as the eye with so many mutually dependent parts, Darwin was able to rejoin that the principle of correlation comes in to ensure that when any one part is improved all other parts shall vary to meet the new conditions. And in general, a knowledge of correlation is necessary in order to complement the study of individual variation and to perfect our investigations upon the origin of species. And correlation must be studied quantitatively. A proper method has been afforded by Galton and Pearson. That method may be briefly stated. Let us suppose that we desire to find the degree of correlation in variation, or deviation from the mean, between an organ A, called subject, and a second organ B, called relative. We first take all the individuals of one (subject) class; that is, individuals whose subject organ deviates from the mean by a constant quantity, p. We next find for those individuals the average deviation-from-the-mean of the organ B, and call it q. We then find the ration q/p; this is the partial index of correlation. We find this ratio for every subject class. The average of the ratios is the index of correlation sought. The ratio will not exceed unity; because q is bound in the long run not to exceed p. When $q = p$, correlation is perfect and is equal to 1. When the index of correlation is zero, correlation is absent; when the index is negative, correlation is inverse and a large organ is associated with a small one. A good example of organs with strong positive correlation is the right and left arm. Inverse correlation is rarer; an example is stature and cephalic index. The results of studying correlation quantitatively are interesting, as showing how intimately bound together the most remote parts of the body are. Take for example the following table of correlation of parts of the human skeleton, from Pearson's 'Grammar of Science.'

Femur and tibia	.81 to .89
Femur and humerus	.84 to .87
Humerus and radius	.74 to .84
Humerus and ulna	.75 to .86
Clavicle and humerus	.44 to .63
Clavicle and scapula	.12 to .16
Stature and femur	.80 to .81
Stature and humerus	.77 to .81
Stature and fore-arm	.37
Stature and cephalic index	—.80
Length and breadth of skull	.29 to .49
Breadth and height of skull	.10 to .34
Length and capacity of skull	.50 to .89
Length x breadth x height and capacity of skull	.70 to .80
Weight and length (babies)	.62 to .64
Weight and stature (adolescents)	.50 to .72
Right and left femur	.96
Right and left first joint of ring finger	.93
First joints of right hand, index and middle fingers	.90
First joints of right hand, index and little fingers	.82
Metacarpal phalanges, right hand, index and middle fingers.	.94
Metacarpal phalanges, right hand index and little fingers. .	.89
Strength of pull and stature	.22 to .30
Strength of pull and weight	.34 to .54

A study of this table shows us how justified was Darwin's contention that the evolution of one organ necessarily means the evolution of many parts of the body.

The modern methods of studying evolution have still another application. It is sometimes said that variation and heredity are the two factors of evolution. Heredity is, however, only a special case of correlated variation; a correlation between parents and offspring or between any two blood relatives. So evolution is reduced to a single factor—variation, simple and correlated.

As a criticism of the new methods of studying variation it has been urged that, after all, they deal not so much with the causes of evolution as with the mere results. To this criticism it may be rejoined that the first step toward the determination of the causes of a phenomenon is a precise knowledge of the limitations and conditions of the phenomenon itself; and this is what the quantitative study of variation gives. Science has been more retarded by wasted efforts to explain erroneous data than by conscientious attempts to discover the precise facts. For when the facts are correctly known the true explanation often follows at once. Even if the explanation does not follow at once the proper direction of experimentation to discover causes is indicated. Statistics tell us not only the exact static condition of species to-day under the varying circumstances of environment; but they will enable us to measure precisely the results of any change in environment, artificially or naturally brought about. We

shall thus be able not only to tell what are the factors of phylogenetic change, but also the rate of such change. We shall get possession of the laws of evolution so that we can not only reconstruct the past, but also predict the future development of a race.

The importance of knowing the methods of evolution is partly theoretical, like the importance of astronomical investigation, and partly practical. For, on the one hand, a rapid and thoroughgoing improvement of the human race can probably be effected only by understand and applying these methods; and on the other hand, the improvement of live-stock and of food plants must depend on a knowledge of the laws of phylogenesis. How appalling is our ignorance, for example, concerning the effect of a mixing of races as contrasted with pure breeding; a matter of infinite importance in a country like ours containing numerous races and subspecies of men. How little do we know of the direct effect of climate on 'blood'; a matter of concern in a land with such diversified geography. In our fast-filling earth all problems will some day be secondary to that of raising more grain or beef to the acre; then at least the biologic-evolutionary problems will be recognized as paramount. It is for us to anticipate in part the future demands on biology. The State Experiment Stations of our day are doing something in this direction, but for the most part in too narrow a fashion. For the future, broad, far-reaching experiments in evolution are required, with a quantitative study of causes and results.

THE COMBATING OF TUBERCULOSIS.

IN THE LIGHT OF THE EXPERIENCE THAT HAS BEEN GAINED IN THE SUCCESSFUL COMBATING OF OTHER INFECTIOUS DISEASES.*

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THE task with which this Congress will have to busy itself is one of the most difficult, but it is also one in which labor is most sure of its reward. I need not point again to the innumerable victims tuberculosis annually claims in all countries, nor to the boundless misery it brings on the families it attacks. You all know that there is no disease which inflicts such deep wounds on mankind as this. All the greater, however, would be the general joy and satisfaction if the efforts that are being made to rid mankind of this enemy, which consumes its inmost marrow, were crowned with success. There are many, indeed, who doubt the possibility of successfully combating this disease, which has existed for thousands of years and has spread all over the world. This is by no means my opinion. This is a conflict into which we may enter with a surely-founded prospect of success, and I will tell you the reasons on which I base this conviction. Only a few decades ago the real nature of tuberculosis was unknown to us; it was regarded as a consequence, as the expression, so to speak, of social misery, and as this supposed cause could not be got rid of by simple means people relied on the probable gradual improvement of social conditions and did nothing. All this is altered now. We know that social misery does indeed go far to foster tuberculosis, but the real cause of the disease is a parasite—that is, a visible and palpable enemy which we can pursue and annihilate, just as we can pursue and annihilate other parasitic enemies of mankind.

Strictly speaking, the fact that tuberculosis is a preventable disease ought to have become clear as soon as the tubercle bacillus was discovered and the properties of this parasite and the manner of its transmission became known. I may add that I, for my part, was aware of the full significance of this discovery from the first, and so will everybody have been who had convinced himself of the causal relation between tuberculosis and the tubercle bacillus. But the strength of a

* An address delivered before the British Congress on Tuberculosis on July 23.

small number of medical men was inadequate to the conflict with a disease so deeply rooted in our habits and customs. Such a conflict requires the cooperation of many, if possible of all, medical men, shoulder to shoulder with the State and the whole population, and now the moment when such cooperation is possible seems to have come. I suppose there is hardly any medical man now who denies the parasitic nature of tuberculosis, and among the non-medical public, too, the knowledge of the nature of the disease has been widely propagated. Another favorable circumstance is that success has recently been achieved in the combating of several parasitic diseases and that we have learned from these examples how the conflict with pestilences is to be carried on. The most important lesson we have learned from the said experience is that it is a great blunder to treat pestilences according to a general scheme. This was done in former times. No matter whether the pestilence in question was cholera, plague, or leprosy, isolation, quarantine, useless disinfection were always resorted to. But now we know that every disease must be treated according to its own special individuality and that the measures to be taken against it must be most accurately adapted to its special nature, to its etiology. We are entitled to hope for success in combating tuberculosis only if we keep this lesson constantly in view. As so extremely much depends just on this point I shall take the liberty to illustrate it by several examples.

The pestilence which is at this moment in the foreground of interest, the bubonic plague, may be instructive to us in several respects. People used to act upon the conviction that a plague patient was in the highest degree a center of infection, and that the disease was transmitted only by plague patients and their belongings. Even the most recent international agreements are based on this conviction. Although, as compared with formerly, we now have the great advantage that we can, with the aid of the microscope and of experiments on animals, recognize every case of plague with absolute certainty, and although the prescribed inspection of ships, quarantine, the isolation of patients, the disinfection of infected dwellings and ships, are carried out with the utmost care, the plague has, nevertheless, been transmitted everywhere, and has in not a few places assumed grave dimensions. Why this has happened we know very well, owing to the experience quite recently gained as to the manner in which the plague is transmitted. It has been discovered that only those plague patients who suffer from plague-pneumonia—a condition which is fortunately infrequent—are centers of infection, and that the real transmitters of the plague are the rats. There is no longer any doubt that in by far the majority of the cases in which the plague has been transmitted by ocean traffic the transmission took place by means of plague among the ship rats. It has also been found that wherever the rats were intentionally or unin-

tentionally exterminated the plague rapidly disappeared; whereas at other places where too little attention had been paid to the rat plague the pestilence continued. This connection between the human plague and the rat plague was totally unknown before, so that no blame attaches to those who devised the measures now in force against the plague if the said measures have proved unavailing. It is high time, however, that this enlarged knowledge of the etiology of the plague should be utilized in international as well as in other traffic. As the human plague is so dependent on the rat plague it is intelligible that protective inoculation and the application of antitoxic serum have had so little effect. A certain number of human beings may have been saved from the disease by that, but the general spread of the pestilence has not been hindered in the least.

With cholera the case is essentially different; it may under certain circumstances be transmitted directly from human beings to other human beings, but its main and most dangerous propagator is water, and therefore in the combating of cholera water is the first thing to be considered. In Germany, where this principle has been acted on, we have succeeded for four years in regularly exterminating the pestilence (which was introduced again and again from the infected neighboring countries) without any obstruction of traffic.

Hydrophobia, too, is not void of instruction for us. Against this disease the so-called protective inoculation proper has proved eminently effective as a means of preventing the outbreak of the disease in persons already infected, but of course such a measure can do nothing to prevent infection itself. The only real way of combating this pestilence is by compulsory muzzling. In this matter also we have had the most satisfactory experience in Germany, but have at the same time seen that the total extermination of the pestilence can be achieved only by international measures, because hydrophobia, which can be very easily and rapidly suppressed, is always introduced again year after year from the neighboring countries.

Permit me to mention only one other disease, because it is etiologically very closely akin to tuberculosis, and we can learn not a little for the furtherance of our aims from its successful combating. I mean leprosy. It is caused by a parasite which greatly resembles the tubercle bacillus. Just like tuberculosis, it does not break out till long after infection and its course is almost slower. It is transmitted only from person to person, but only when they come into close contact, as in small dwellings and bedrooms. In this disease, accordingly, immediate transmission plays the main part; transmission by animals, water, or the like is out of the question. The combative measures, accordingly, must be directed against this close intercourse between the sick and the healthy. The only way to prevent this intercourse is to isolate the

patients. This was most rigorously done in the Middle Ages by means of numerous leper-houses, and the consequence was that leprosy, which had spread to an alarming extent, was completely stamped out in Central Europe. The same method has been adopted quite recently in Norway, where the segregation of lepers has been ordered by a special law. But it is extremely interesting to see how this law is carried out. It has been found that it is not at all necessary to execute it strictly, for the segregation of only the worst cases, and even of only a part of these, sufficed to produce a diminution of leprosy. Only so many infectious cases had to be sent to the leper-houses that the number of fresh cases kept regularly diminishing from year to year. Consequently the stamping-out of the disease has lasted much longer than it would have lasted if every leper had been inexorably consigned to a leper-house, as in the Middle Ages, but in this way, too, the same purpose is gained, slowly indeed, but without any harshness.

These examples may suffice to show what I am driving at, which is to point out that in combating pestilences we must strike the root of the evil and must not squander force in subordinate ineffective measures. Now the question is whether what has hitherto been done and what is about to be done against tuberculosis really strikes the root of tuberculosis so that it must sooner or later die. In order to answer this question it is necessary first and foremost to inquire how infection takes place in tuberculosis. Of course, I presuppose that we understand by tuberculosis only those morbid conditions which are caused by the tubercle bacillus. In by far the majority of cases of tuberculosis the disease has its seat in the lungs, and has also begun there. From this fact it is justly concluded that the germs of the disease—*i. e.,* the tubercle bacilli—must have got into the lungs by inhalation. As to the question where the inhaled tubercle bacilli have come from, there is also no doubt. On the contrary, we know with certainty that they get into the air with the sputum of consumptive patients. This sputum, especially in advanced stages of the disease, almost always contains tubercle bacilli, sometimes in incredible quantities. By coughing and even speaking it is flung into the air in little drops—*i. e.,* in a moist condition—and can at once infect persons who happen to be near the coughers. But then it may also be pulverized when dry, in the linen or on the floor for instance, and get into the air in the form of dust. In this manner a complete circle, a so-called *circulus vitiosus,* has been formed for the process of infection from the diseased lung, which produces phlegm and pus containing tubercle bacilli, to the formation of moist and dry particles (which in virtue of their smallness can keep floating a good while in the air), and finally to new infection if particles penetrate with the air into a healthy lung and originate the disease anew. But the tubercle bacilli may get to other organs of the

body in the same way and thus originate other forms of tuberculosis. This, however, is a considerably rarer case. The sputum of consumptive people, then, is to be regarded as the main source of the infection of tuberculosis. On this point, I suppose, all are agreed. The question now arises whether there are not other sources, too, copious enough to demand consideration in the combating of tuberculosis.

Great importance used to be attached to the hereditary transmission of tuberculosis. Now, however, it has been demonstrated by thorough investigation that, though hereditary tuberculosis is not absolutely non-existent, it is nevertheless extremely rare, and we are at liberty in considering our practical measures to leave this form of origination entirely out of account. But another possibility of tuberculous infection exists, as is generally assumed, in the transmission of the germs of the disease from tuberculous animals to man. This manner of infection is generally regarded nowadays as proved and as so frequent that it is even looked upon by not a few as the most important, and the most rigorous measures are demanded against it. In this Congress also the discussion of the danger with which the tuberculosis of animals threatens man will play an important part. Now, as my investigations have led me to form an opinion deviating from that which is generally accepted, I beg your permission, in consideration of the great importance of this question, to discuss it a little more thoroughly.

Genuine tuberculosis has hitherto been observed in almost all domestic animals, and most frequently in poultry and cattle. The tuberculosis of poultry, however, differs so much from human tuberculosis that we may leave it out of account as a possible source of infection for man. So, strictly speaking, the only kind of tuberculosis remaining to be considered is the tuberculosis of cattle which, if really transferable to man, would indeed have frequent opportunities of infecting human beings through the drinking of the milk and the eating of the flesh of diseased animals. Even in my first circumstantial publication on the etiology of tuberculosis I expressed myself regarding the identity of human tuberculosis and bovine tuberculosis with reserve. Proved facts which would have enabled me sharply to distinguish these two forms of the disease were not then at my disposal, but sure proofs of their absolute identity were equally undiscoverable, and I therefore had to leave this question undecided. In order to decide it I have repeatedly resumed the investigations relating to it, but so long as I experimented on small animals, such as rabbits and guinea pigs, I failed to arrive at any satisfactory result, though indications which rendered the difference of the two forms of tuberculosis probable were not wanting. Not till the complaisance of the Ministry of Agriculture enabled me to experiment on cattle, the only animals really suitable for these investigations, did I arrive at absolutely conclusive results.

Of the experiments which I have carried out during the last two years along with Professor Schütz of the Veterinary College in Berlin I will tell you briefly some of the most important.

A number of young cattle which had stood the tuberculin test, and might therefore be regarded as free from tuberculosis, were infected in various ways with tubercle bacilli taken from cases of human tuberculosis; some of them got the tuberculous sputum of consumptive patients direct. In some cases the tubercle bacillus or the sputum was injected under the skin, in others into the peritoneal cavity, in others into the jugular vein. Six animals were fed with tuberculous sputum almost daily for seven or eight months; four repeatedly inhaled great quantities of bacilli, which were distributed in water and scattered with it in the form of spray. None of these cattle (there were 19 of them) showed any symptoms of disease and they gained considerably in weight. From six to eight months after the beginning of the experiments they were killed. In their internal organs not a trace of tuberculosis was found. Only at the places where the injections had been made small suppurative foci had formed, in which few tubercle bacilli could be found. This is exactly what is found when dead tubercle bacilli are injected under the skin of animals liable to contagion. So the animals we experimented on were affected by the living bacilli of human tuberculosis exactly as they would have been by dead ones; they were absolutely insusceptible to them.

The result was utterly different, however, when the same experiment was made on cattle free from tuberculosis with tubercle bacilli that came from the lungs of an animal suffering from bovine tuberculosis. After an incubation-period of about a week the severest tuberculous disorders of the internal organs broke out in all the infected animals. It was all one whether the infecting matter had been injected only under the skin or into the peritoneal cavity or the vascular system. High fever set in and the animals became weak and lean; some of them died after from one and a half to two months; others were killed in a miserably sick condition after three months. After death extensive tuberculous infiltrations were found at the place where the injections had been made and in the neighboring lymphatic glands, and also far-advanced alterations of the internal organs, especially of the lungs and the spleen. In the cases in which the injection had been made into the peritoneal cavity the tuberculous growths which are so characteristic of bovine tuberculosis were found on the omentum and peritoneum. In short, the cattle proved just as susceptible to infection by the bacillus of bovine tuberculosis as they have proved insusceptible to infection by the bacillus of human tuberculosis. I wish only to add that preparations of the organs of the cattle which were artificially infected with bovine tuberculosis in these experiments are exhibited in the museum of pathology and bacteriology.

An almost equally striking distinction between human and bovine tuberculosis was brought to light by a feeding experiment with swine. Six young swine were fed daily for three months with the tuberculous sputum of consumptive patients. Six other swine received bacilli of bovine tuberculosis with their food daily for the same period. The animals that were fed with sputum remained healthy and grew lustily, whereas those that were fed with the bacilli of bovine tuberculosis soon became sickly, were stunted in their growth, and half of them died. After three and a half months the surviving swine were all killed and examined. Among the animals that had been fed with sputum no trace of tuberculosis was found, except here and there little nodules in the lymphatic glands of the neck and in one case a few grey nodules in the lungs. The animals, on the other hand, which had eaten bacilli of bovine tuberculosis had, without exception (just as in the cattle experiment), severe tuberculous diseases, especially tuberculous infiltration of the greatly enlarged lymphatic glands of the neck and of the mesenteric glands, and also extensive tuberculosis of the lungs and the spleen.

The difference between human and bovine tuberculosis appeared not less strikingly in a similar experiment with asses, sheep and goats, into whose vascular systems the two kinds of tubercle bacilli were injected.

Our experiments, I must add, are not the only ones that have led to this result. If one studies the older literature of the subject, and collates the reports of the numerous experiments that were made in former times by Chauveau, Günther and Harms, Bollinger and others, who fed calves, swine and goats with tuberculous material, one finds that the animals that were fed with the milk and pieces of the lungs of tuberculous cattle always fell ill of tuberculosis, whereas those that were fed with human material did not. Comparative investigations regarding human and bovine tuberculosis have been made very recently in North America by Smith, Dinwiddie, Frothingham and Repp, and their result agreed with that of ours. The unambiguous and absolutely conclusive result of our experiments is due to the fact that we chose methods of infection which excluded all sources of error, and carefully avoided everything connected with the stalling, feeding and tending of the animals that might have a disturbing effect on the experiments. Considering all these facts, I feel justified in maintaining that human tuberculosis differs from bovine and cannot be transmitted to cattle. It seems to me very desirable, however, that these experiments should be repeated elsewhere, in order that all doubts as to the correctness of my assertion may be removed. I wish only to add that, owing to the great importance of this matter, our Government has resolved to appoint a commission to make further inquiries on the subject.

But, now, how is it with the susceptibility of man to bovine tuberculosis? This question is far more important to us than that of the susceptibility of cattle to human tuberculosis, highly important as that is too. It is impossible to give this question a direct answer, because, of course, the experimental investigation of it with human beings is out of the question. Indirectly, however, we can try to approach it. It is well known that the milk and butter consumed in great cities very often contain large quantities of the bacilli of bovine tuberculosis in a living condition, as the numerous infection-experiments with such dairy products on animals have proved. Most of the inhabitants of such cities daily consume such living and perfectly virulent bacilli of bovine tuberculosis, and unintentionally carry out the experiment which we are not at liberty to make. If the bacilli of bovine tuberculosis were able to infect human beings, many cases of tuberculosis caused by the consumption of alimenta containing tubercle bacilli could not but occur among the inhabitants of great cities, especially the children. And most medical men believe that this is actually the case.

In reality, however, it is not so. That a case of tuberculosis has been caused by alimenta can be assumed with certainty only when the intestine suffers first—*i. e.,* when a so-called primary tuberculosis of the intestines is found. But such cases are extremely rare. Among many cases of tuberculosis examined after death I myself remember having seen primary tuberculosis of the intestine only twice. Among the great post-mortem material of the Charité Hospital in Berlin 10 cases of primary tuberculosis of the intestine occurred in five years. Among 933 cases of tuberculosis in children at the Emperor Frederick's Hospital for Children Baginsky never found tuberculosis of the intestine without simultaneous disease of the lungs and the bronchial glands. Among 3,104 post-mortem examinations of tuberculous children Biedert observed only 16 cases of primary tuberculosis of the intestine. I could cite from the literature of the subject many more statistics of the same kind, all indubitably showing that primary tuberculosis of the intestine, especially among children, is a comparatively rare disease, and of the few cases that have been enumerated it is by no means certain that they were due to infection by bovine tuberculosis. It is just as likely that they were caused by the widely-propagated bacilli of human tuberculosis, which may have got into the digestive canal in some way or other—for instance, by swallowing saliva of the mouth. Hitherto nobody could decide with certainty in such a case whether the tuberculosis of the intestine was of human or of animal origin. Now we can diagnose the two. All that is necessary is to cultivate in pure culture the tubercle bacilli found in the tuberculous material and to ascertain whether they belong to bovine tuberculosis by inoculating cattle with them. For this purpose I recom-

mend subcutaneous injection which yields quite specially characteristic and convincing results. For half a year past I have occupied myself with such investigations, but owing to the rareness of the disease in question the number of the cases which I have been able to investigate is but small. What has hitherto resulted from this investigation does not speak for the assumption that bovine tuberculosis occurs in man.

Though the important question whether man is susceptible to bovine tuberculosis at all is not yet absolutely decided, and will not admit of absolute decision to-day or to-morrow, one is nevertheless already at liberty to say that, if such a susceptibility really exists the infection of human beings is but a very rare occurrence. I should estimate the extent of infection by the milk and flesh of tuberculous cattle and the butter made of their milk as hardly greater than that of hereditary transmission, and I therefore do not deem it advisable to take any measures against it.

So the only main source of the infection of tuberculosis is the sputum of consumptive patients and the measures for the combating of tuberculosis must aim at the prevention of the dangers arising from its diffusion. Well, what is to be done in this direction? Several ways are open. One's first thought might be to consign all persons suffering from tuberculosis of the lungs whose sputum contains tubercle bacilli to suitable establishments. This, however, is not only absolutely impracticable but also unnecessary. For a consumptive who coughs out tubercle bacilli is not necessarily a source of infection on that account so long as he takes care that his sputum is properly removed and rendered innocuous. This is certainly true of very many patients, especially in the first stages, and also of those who belong to the well-to-do classes and are able to procure the necessary nursing. But how is it with people of very small means? Every medical man who has often entered the dwellings of the poor, and I can speak on this point from my own experience, knows how sad is the lot of consumptives and their families there. The whole family have to live in one or two small, ill-ventilated rooms. The patient is left without the nursing he needs because the able-bodied members of the family must go to their work. How can the necessary cleanliness be secured under such circumstances? How is such a helpless patient to remove his sputum so that it may do no harm? But let us go a step further and picture the condition of a poor consumptive patient's dwelling at night. The whole family sleep crowded together in one small room. However cautious he may be the sufferer scatters the morbid matter secreted by his diseased lungs every time he coughs and his relatives close beside him must inhale this poison. Thus whole families are infected. They die out and awaken in the minds of those who do not know the infectious-

ness of tuberculosis the opinion that it is hereditary, whereas its transmission in the cases in question was due solely to the simplest processes of infection, which do not strike people so much because the consequences do not appear at once, but generally only after the lapse of years.

Often in such circumstances the infection is not restricted to a single family, but spreads in densely inhabited tenement houses to the neighbors, and then, as the admirable investigations of Biggs have shown in the case of the densely peopled parts of New York, regular nests or foci of disease are formed. But if one investigates these matters more thoroughly one finds that it is not poverty *per se* that favors tuberculosis, but the bad domestic conditions under which the poor everywhere, but especially in great cities, have to live. For, as the German statistics show, tuberculosis is less frequent, even among the poor, when the population is not densely packed together, and may attain very great dimensions among a well-to-do population when the domestic conditions, especially as regards the bedrooms, are bad, as is the case, for instance, among the inhabitants of the North Sea coast. So it is the overcrowded dwellings of the poor that we have to regard as the real breeding-places of tuberculosis; it is out of them that the disease always crops up anew, and it is to the abolition of these conditions that we must first and foremost direct our attention if we wish to attack the evil at its root and to wage war against it with effective weapons. This being so, it is very gratifying to see how efforts are being made in almost all countries to improve the domestic conditions of the poor. I am also convinced that these efforts, which must be promoted in every way, will lead to a considerable diminution of tuberculosis. But a long time must elapse ere essential changes can be effected in this direction, and much may be done meanwhile in order to reach the goal much more rapidly.

If we are not able at present to get rid of the danger which small and overcrowded dwellings involve, all we can do is to remove the patients from them and, in their own interests and that of the people about them, to lodge them better, and this can be done only in suitable hospitals. But the thought of attaining this end by compulsion of any kind is very far from me; what I want is that they may be enabled to obtain the nursing they need better than they can obtain it now. At present a consumptive in an advanced stage of the disease is regarded as incurable and as an unsuitable inmate for a hospital. The consequence is that he is reluctantly admitted and dismissed as soon as possible. The patient, too, when the treatment seems to him to produce no improvement and the expenses, owing to the long duration of his illness, weigh heavily upon him, is himself animated by the wish to leave the hospital soon. That would be altogether altered if

we had special hospitals for consumptives, and if the patients were taken care of there for nothing, or at least at a very moderate rate. To such hospitals they would willingly go; they could be better treated and fed there than is now the case. I know very well that the execution of the project will have great difficulties to contend with, owing to the considerable outlay it entails. But very much would be gained if, at least in the existing hospitals, which have to admit a great number of consumptives at any rate, special wards were established for them in which pecuniary facilities would be offered them. If only a considerable fraction of the whole number of consumptives were suitably lodged in this way a diminution of infection, and consequently of the sum-total of tuberculosis, could not fail to be the result. Permit me to remind you in this connection of what I said about leprosy. In the combating of that disease also great progress has already been made by lodging only a fair number of the patients in hospitals. The only country that possesses a considerable number of special hospitals for tuberculous patients is England, and there can be no doubt that the diminution of tuberculosis in England, which is much greater than in any other country, is greatly due to this circumstance. I should point to the founding of special hospitals for consumptives and the better utilization of the already existing hospitals for the lodging of consumptives as the most important measure in the combating of tuberculosis, and its execution opens a wide field of activity to the State, to municipalities, and to private benevolence. There are many people who possess great wealth and would willingly give of their superfluity for the benefit of their poor and heavily afflicted fellow-creatures, but do not know how to do this in a judicious manner. Here is an opportunity for them to render a real and lasting service by founding consumption hospitals or purchasing the right to have a certain number of consumptive patients maintained in special wards of other hospitals free of expense.

As, however, unfortunately, the aid of the State, the municipalities, and rich benefactors will probably not be forthcoming for a long time yet, we must for the present resort to other measures that may pave the way for the main measure just referred to and serve as a supplement and temporary substitute for it. Among such measures I regard obligatory notification as specially valuable. In the combating of all infectious diseases it has proved indispensable as a means of obtaining certain knowledge as to their state, especially their dissemination, their increase, and their decrease. In the conflict with tuberculosis also we cannot dispense with obligatory notification; we need it not only in order to inform ourselves as to the dissemination of this disease, but mainly in order to learn where help and instruction can be given, and especially where the disinfection which is so urgently

necessary when consumptives die or change their residences has to be effected. Fortunately it is not at all necessary to notify all cases of tuberculosis, nor even all cases of consumption, but only those that, owing to the domestic conditions, are sources of danger to the people about them. Such limited notification has already been introduced in various places—in Norway, for instance, by a special law, in Saxony by a Ministerial decree, in New York, and in several American towns which have followed its example. In New York, where notification was optional at first and was afterwards made obligatory, it has proved eminently useful. It has thus been proved that the evils which it used to be feared the introduction of notification for tuberculosis would bring about need not occur and it is devoutly to be wished that the examples I have named may very soon excite emulation everywhere.

There is another measure connected with notification—viz., disinfection, which, as already mentioned, must be effected when consumptives die or change their residence in order that those who next occupy the infected dwelling may be protected against infection. Moreover, not only the dwellings but also the infected beds and clothes of consumptives ought to be disinfected. A further measure, already recognized on all hands as effective, is the instructing of all classes of the people as to the infectiousness of tuberculosis and the best way of protecting oneself. The fact that tuberculosis has considerably diminished in almost all civilized states of late is attributable solely to the circumstance that knowledge of the contagious character of tuberculosis has been more and more widely disseminated and that caution in intercourse with consumptives has increased more and more in consequence. If better knowledge of the nature of tuberculosis has alone sufficed to prevent a large number of cases this must serve us as a significant admonition to make the greatest possible use of this means and to do more and more to bring it about that everybody may know the dangers that threaten them in intercourse with consumptives. It is only to be desired that the instructions may be made shorter and more precise than they generally are, and that special emphasis may be laid on the avoidance of the worst danger of infection, which is the use of bedrooms and small ill-ventilated workrooms simultaneously with consumptives. Of course the instructions must include directions as to what consumptives have to do when they cough and how they are to treat their sputum. Another measure, which has come into the foreground of late, and which at this moment plays to a certain extent a paramount part in all efforts for the combating of tuberculosis, works in quite another direction. I mean the founding of sanatoria for consumptives.

That tuberculosis is curable in its early stages must be regarded as an undisputed fact. The idea of curing as many tuberculous

patients as possible in order to reduce the number of those that reach the infectious stage of consumption and thus to reduce the number of fresh cases was therefore a very natural one. The only question is whether the number of persons cured in this way will be great enough to exercise an appreciable influence on the retrogression of tuberculosis. I will try to answer this question in the light of the figures at my disposal. According to the business report of the German Central Committee for the Establishment of Sanatoria for the Cure of Consumptives, about 5,500 beds will be at the disposal of these institutions by the end of 1901, and then, if we assume that the average stay of each patient will be three months, it will be possible to treat at least 20,000 patients every year. From the reports hitherto issued as to the results that have been achieved in the establishments we learn further that about 20 per cent. of the patients who have tubercle bacilli in their sputum lose them by the treatment there. This is the only sure test of success, especially as regards prophylaxis. If we make this the basis of our estimates, we find that 4,000 consumptives will leave these establishments annually as cured. But, according to the statistics ascertained by the German Imperial Office of Health, there are 226,000 persons in Germany over fifteen years of age who are so far gone in consumption that hospital treatment is necessary for them. Compared with this great number of consumptives the success of the establishments in question seems so small that a material influence on the retrogression of tuberculosis in general is not yet to be expected of them. But pray do not imagine that I wish by this calculation of mine to oppose the movement for the establishment of such sanatoria in any way. I only wish to warn against the overestimating of their importance which has recently been observable in various quarters, based apparently on the opinion that the war against tuberculosis can be waged by means of sanatoria alone and that other measures are of subordinate value. In reality the contrary is the case. What is to be achieved by the general prophylaxis resulting from recognition of the danger of infection and the consequent greater caution in intercourse with consumptives is shown by a calculation of Cornet's regarding the decrease of mortality from tuberculosis in Prussia in the years 1889 to 1897. Before 1889 the average was 31.4 per 10,000, whereas in the period named it sank to 21.8, which means that in that short space of time the number of deaths from tuberculosis was 184,000 less than was to be expected from the average of the preceding years. In New York, under the influence of the general sanitary measures directed in a simply exemplary manner by Biggs, the mortality from tuberculosis has diminished by more than 35 per cent. since 1886. And it must be remembered that both in Prussia and in New York the progress indi-

cated by these figures is due to the first beginnings of these measures. Considerably greater success is to be expected of their further development. Biggs hopes to have got so far in five years that in the city of New York alone the annual number of deaths from tuberculosis will be 3,000 less than formerly.

Now, I do indeed believe that it will be possible to render the sanatoria considerably more efficient. If strict care be taken that only patients be admitted for whom the treatment of those establishments is well adapted and if the duration of the treatment be prolonged it will certainly be possible to cure 50 per cent. and perhaps still more. But even then, and even if the number of the sanatoria be greatly increased, the total effect will always remain but moderate. The sanatoria will never render the other measures I have mentioned superfluous. If their number becomes great, however, and if they perform their functions properly, they may materially aid the strictly sanitary measures in the conflict with tuberculosis.

If now, in conclusion, we glance back once more to what has been done hitherto for the combating of tuberculosis, and forward to what has still to be done, we are at liberty to declare with a certain satisfaction that very promising beginnings have already been made. Among these I reckon the consumption hospitals of England, the legal regulations regarding notification in Norway and Saxony, the organization created by Biggs in New York (the study and imitation of which I most urgently recommend to all municipal sanitary authorities), the sanatoria, and the instruction of the people. All that is necessary is to go on developing these beginnings, to test and, if possible, to increase their influence on the diminution of tuberculosis, and wherever anything useful has yet been done to do likewise. If we allow ourselves to be continually guided in this enterprise by the spirit of genuine preventive medical science, if we utilize the experience gained in conflict with other pestilences, and aim, with clear recognition of the purpose and resolute avoidance of wrong roads, at striking the evil at its root, then the battle against tuberculosis, which has been so energetically begun, cannot fail to have a victorious issue.

THE DISCOVERY OF THE LAW OF GRAVITATION.

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THE law of gravitation, that all matter attracts all matter, directly as the mass and inversely as the square of the distance, whether we consider the extent of its reach, or the number and variety and peculiar interest of the problems of which it furnishes the solution, or the grandeur of many of those problems by reason of the magnitude of the elements involved; whether we consider the power which it gives us to anticipate nature, and predict with the minutest accuracy and certainty of a mathematical demonstration celestial phenomena for ages to come; we cannot but regard it as the most important truth in the whole book of nature and its discovery as the most interesting event in the history of physical science. As there is but one material universe and the law of gravitation solves the enigma of *its* structure, no other problem of equal interest and importance can ever occupy the attention of the student of nature.

Kepler has remarked that: "The occasions by which men have acquired a knowledge of celestial phenomena, are not less admirable than the discoveries themselves." If this be so, the history of the discovery of that great law of nature by which all celestial phenomena are determined can never cease to be a matter of peculiar interest.

In the account which we propose to give of the discovery we shall select as our chronological starting point the beginning of the seventeenth century. At that period the theory in regard to the structure of the material universe which, with few exceptions had been held from time immemorial, still prevailed. The earth was regarded as the center of the universe, about which the sun, moon, planets and stars performed their ceaseless revolutions. More than half a century before (in 1543) Copernicus, in his memorable work, 'De Orbium Cœlestium Revolutionibus,' had, indeed, announced the true system of the universe, yet as he was led to the adoption of the theory he proposed, not so much by positive evidence in its favor as by the difficulty of reconciling certain phenomena with the Ptolemaic theory; moreover, as the objections to this theory were from their very nature such that few could appreciate their force, whilst in the *apparent* motions of the heavenly bodies every one could see what seemed to be an ocular demonstration of its truth, it is not strange that the doctrine of Copernicus should have been for so long a time generally regarded as an

interesting yet fanciful speculation. It remained for a subsequent age to furnish proof of the truth of the Copernican system which could not be gainsaid or resisted.

At the beginning of the seventeenth century, the dicta of Aristotle, in regard to matters of science as well as philosophy, were still accepted, as they had been for many centuries preceding, as of infallible authority. In regard to the subject of our inquiry, he taught that bodies at the surface of the earth fell or tended to fall toward the center of the earth, not in virtue of any attraction of the earth, but in virtue of the fact that the center of the earth was the center of the material universe —that if the earth itself should be moved out of its place and then left free to move, it would return to its place by the same law of nature which controlled all terrestrial bodies. He taught moreover that celestial bodies were different in kind from bodies terrestrial—that whilst the latter were imperfect, corruptible and changeable, the former were perfect, (and therefore, according to his fancy, perfectly spherical in form) incorruptible, unchangeable and self-luminous. Being different in kind, he held that they were subject to entirely different physical laws; that whereas the motion of terrestrial bodies when free to move was rectilinear, by a necessity of their nature, the motion of celestial bodies was circular by a like necessity of their nature. His language on this point is worth quoting as an illustration of the contrast between the ancient and modern method of philosophizing in regard to natural phenomena. He says: "All simple motion must be rectilinear or circular; either to a center or from a center, each of which is rectilinear, or about a center. It is natural for two of the elements—earth and water—which are heavy, to tend to a center; two—air and fire—which are light, to tend from a center. As the motion of all terrestrial elements is therefore rectilinear, it seems reasonable that celestial bodies, which are of a different nature, should have only the other simple motion possible, namely, circular motion."

The year 1609 marks a new era in the history of astronomy. In this year two events occurred, independent, yet alike memorable as contributing to the overthrow of the theory in regard to the structure of the material universe which had previously prevailed and establishing the doctrine of Copernicus upon an immovable foundation. The invention of the telescope by Galileo, and the immediate discovery by means of it of the inequalities of the moon's surface, the phases of Venus, the satellites of Jupiter and the rings of Saturn, at once annihilated the fancies of Aristotle as to the perfectly spherical form of the planets, their self-luminosity, and their difference in kind from bodies terrestrial. The other memorable event referred to was the publication of Kepler's great work on 'The Motions of Mars,' in which, with much that was fanciful, two of the three laws of planetary motion were for

the first time announced. Some twelve years later, in his work, entitled 'Harmonies,' he announced the third law of planetary motion, fully establishing his right to the title, by which he has since been distinguished 'The Legislator of the Heavens.'

These laws of Kepler are: First, that the orbits of the planets are elliptical, the sun being at one of the foci; second, that the radius vector, that is, a line drawn from a planet to the sun, passes over equal spaces in equal times; third, that the squares of the times of revolution of the different planets, are to each other as the cubes of the mean distances from the sun.

Together with these laws of planetary motion, two of the three axioms of the science of mechanics, known as *the Laws of Motion,* were about this time discovered, or rather, were now for the first time distinctly apprehended and enunciated. The first of these was given by Kepler—the law of *inertia,* namely, that a body will persevere in the state in which it is, whether of rest or motion, until it is acted on by some force; or more precisely, a body at rest will continue at rest until acted on by some force, and when acted on by any single force, if free to move, its motion will be rectilinear, uniform and continuous until the body is acted on by some other force. The second law of motion was announced by Galileo, and is known as the law of *the coexistence of motions,* or independence of forces. It may be expressed as follows: If a body be acted on by several forces simultaneously, it will obey the impulse of each force, just as it would if the others were not acting. [The simplest illustration of this law is what is known as the parallelogram of forces. If the direction and intensity of two forces acting simultaneously on a body be represented by the sides of a parallelogram, the body will describe the diagonal of the parallelogram; that is, at the end of a unit of time the body will be just where it would have been if the forces had, each for a unit of time, acted consecutively.]

The true system of the universe, the laws of planetary motion and the fundamental principles of mechanics having become known, for the first time in the history of the race any intelligent inquiry as to the physical causes of the motions of the heavenly bodies became possible. With earnestness and assiduity proportioned to the interest and grandeur of the problem, men of science at once applied themselves to its solution, and yet half a century of gradual progress elapsed before the desired result was reached. From the facts which we shall have occasion to mention it will appear how much, or rather how little, foundation there is for the common belief that the idea of the law of gravitation was wholly original with Newton—suggested to him for the first time by observing the fall of an apple, and then suddenly coming forth from his brain like Minerva from the head of Jove, un-

heralded and complete. The ordinary method of transition from widespread and plausible error to the truth is by slow and gradual progress, and the discovery of the law of gravitation, so far from being an exception to this rule, is but one of its most striking illustrations. Such an *accident* as that which the discovery of the law of gravitation is supposed to have been is of the kind which only happens to men of large knowledge, profound thought, and after intense and protracted mental effort. Simple as this law is known to be, and easily apprehended and even demonstrated by ordinary minds, it needed one endowed with the most gigantic intellect probably ever given to mortal—availing himself of the suggestions and the results of the labors of those who had preceded him in the same field of inquiry—to make the discovery.

In tracing the history of this discovery, from the epoch when by the previous discovery of all the necessary data it for the first time became possible, the first place in the order of time, and next to Newton in the order of merit, is undoubtedly due to Kepler. Possessing a singularly lively imagination—we might say, volatile fancy—combined with a love for the truth that amounted to a ruling passion, and a breadth of knowledge in his favorite science far in advance of any other man of his age, he was eminently fitted for the work which he so successfully performed of scientific discovery. Fertile in hypotheses—sometimes the most extravagant—he was indefatigable in his labors to test his hypotheses by the facts. Without the slightest pride of opinion, he seemed to take a satisfaction in exploding his own theories when they were false, that was only exceeded by his delight when successful in demonstrating their truth. Of the men who have contributed to the advancement of science, there are few to whom we are under greater obligation, or whose character as an investigator of nature is more worthy of admiration, than 'The Legislator of the Heavens'—the father of modern astronomy.

In the introduction to his memorable work on 'The Motions of Mars,' referred to above, he opposed the doctrine of Aristotle on the subject of terrestrial gravity, and in the course of the discussion uses the following remarkable language:

> A mathematical point, whether the center of the universe, or not, has no power to move heavy bodies to approach it. Let philosophers prove, if they can, that natural things have any sympathy with that which is nothing.
>
> The true theory of gravity is founded on the following axioms. *Gravity is a mutual affection between cognate bodies toward union or conjunction, similar to the magnetic virtue.* If we assume the earth to be the center of the world, heavy bodies are not carried toward its center in virtue of its quality of center of the world, but in virtue of its quality of center of a cognate round body; so that wheresoever the earth may be placed, or whithersoever it may be carried by its animal faculty (alluding to a fanciful theory which we shall have occasion presently to notice) heavy bodies will always be carried toward it.

If the earth were not round, heavy bodies would not tend from every side toward its center, but to different points from different sides.

If two stones were placed in any part of the universe, near each other, and beyond the sphere of the influence of a third cognate body, these stones would come together at an intermediate point, each approaching the other at a distance proportional to the comparative mass of the other.

If the moon and the earth were not retained in their orbits by their annual force, or some other equivalent, the earth would mount to the moon by a fifty-fourth part of their distance from each other, and the moon would fall toward the earth through the other fifty-three parts, that is, assuming that the substance of the earth is of the same density.

The sphere of the attractive virtue which is in the moon, extends to the earth and entices up the waters, but as the moon flies rapidly across the zenith and the waters cannot follow so quickly, a flow of the ocean is occasioned toward the westward.

If the attractive virtue of the moon extends to the earth, it follows, with greater reason, that the attractive virtue of the earth extends to the moon and much farther, and in short, nothing which consists of earthy substance, however constituted, although thrown up to any height, can ever escape the powerful operation of this attractive virtue.

These views of Kepler—so novel at the time they were announced by him and yet which we now know to be in the main so correct—were published more than thirty years before Newton was born. As we read them, our first feeling is one of surprise that any subsequent investigator of the phenomena of gravitation should be able, by his discoveries, to achieve for himself a fame which should not only render his name immortal but should almost wholly hide from view the merit of the great pioneer in this field of inquiry. To appreciate, however, the importance of the work which yet remained to be performed, we should bear in mind that whilst Kepler's views in regard to terrestrial gravity were so remarkably just, he at the same time, in common with the age in which he lived, and, we may say, with all preceding ages—regarded the tendency of bodies near the earth to fall toward its center, and the motions of heavenly bodies, as entirely different phenomena and not at all referable to the same physical cause. He indeed speculated on the possibility of referring the motions of the planets to an attractive force emanating from the sun, similar to that which caused bodies near the earth to tend toward its center, and concluded that such a hypothesis was untenable, inasmuch as the motion in one case was rectilinear, and in the other curvilinear. Again, not to overestimate the merit of Kepler in connection with the discovery of the law of gravitation, we should remember that a theory as to the physical cause of natural phenomena, even if it be in the main correct, will furnish no complete solution of the problems which phenomena present, unless it express accurately and precisely the *measure* as well as the *mode* of the action of the assigned cause. For example, to know merely that all matter attracts all matter, would not enable us to

explain the phenomena of gravitation; we need to know precisely how the intensity of this attraction is affected by the comparative magnitude of the masses and by the distance of the masses from each other. Now the theory of Kepler in regard to gravity was correct as to the first of these points, namely that the intensity of this attraction was directly as the mass, but he was in error in regard to the second point, as he supposed that the intensity of the attraction was inversely as the distance, instead of what was subsequently found to be the fact, the square of the distance.

Once more, to estimate at its just value the part which Kepler performed in the discovery of the laws of gravitation, we should bear in mind, that an hypothesis, even if subsequently it be found to be correct, is of no *authority* until its truth be demonstrated. It may be of great importance, by way of suggestion, in directing the labors of subsequent inquirers, but the chief merit of the discovery of the truth is due to the individual who furnishes its demonstration. When this is done, and not before, that which was previously but an hypothesis takes its place among the recognized laws of nature.

As in Kepler's day, the tendency of bodies near the earth to fall toward its center and the motions of the heavenly bodies were regarded as phenomena of entirely different laws of nature, his views as to the physical cause of planetary motion next claim our attention. He supposed that the motion of the planets in their orbits was due to an influence emanating from the sun, but assuming that if this influence were an *attractive* force, similar to terrestrial gravity, its effect would be to cause the planets to fall toward the sun in straight lines, instead of the actual motion of revolution about the sun; he supposed that the emanation was of a corporeal nature, somewhat analogous to light; that as the sun revolved on its axis, this emanation revolved with it just as the spokes of a wheel when the hub revolves, and that the planets were swept along in their orbits by the revolution of this emanation—the force which caused them to move being a *propulsion* and not an *attraction*. As the hypothesis would seem to require that the times of revolution of all the planets should be the same, whereas they are different, the nearer performing their annual revolution in a time less than the more remote—he supposed that the density of the emanation diminished as its distance from the sun increased; that consequently its virtue, or propulsive energy, diminished in like manner, just as the intensity of light diminishes with the increase of the distance from the luminous center. This would account in a general way for the fact that the times of revolution of the planets nearer the sun are shorter than the times of revolutions of those more remote, but the precise difference in the observed times of revolution was not exactly that which would be required by the hypothesis. Moreover, he had observed

that the orbits of the planets were not circular, as would seem to be required by his hypothesis, but elliptical, the sun being at one of the foci; also, the ever-varying radius vector always passed over equal spaces in equal times, hence the motion of the planet in its orbit was not uniform, as his hypothesis would require, but ever-varying; and this variation too was evidently not fortuitous or uncertain but increased or diminished in the exact ratio to the varying distance of the planet from the sun required by the law just mentioned, of equal spaces in equal times. These facts, apparently so inconsistent with his hypothesis, Kepler accounted for by supposing that each of the planets was animated by an intelligent spirit, by whose agency the motion of the planet was, in part at least, determined. We have seen an allusion to this theory in the quotation above given, on the subject of gravity. He regarded each of the heavenly bodies, and the earth as one of them, as literally a huge animal, and in one of his works describes with some minuteness the habits of that particular animal on whose body it is our lot to live.

Kepler's hypothesis of an emanation from the sun of a corporeal nature by whose revolution the planets were propelled in their orbits was received with more or less favor for a time, but was soon superseded by another memorable hypothesis no more reasonable or plausible and yet from the time of its announcement until the publication of 'The Principia' demonstrated its fallacy, it was adopted by most men of science and may be said to have been the accepted theory on the subject. We refer to the *Vortices of Descartes.* This distinguished philosopher, born 1596, rose to eminence about the time of Kepler's death, which occurred in 1630. By the force of his genius, illustrated not only by that achievement for which his name will ever be held in honored remembrance—the invention of analytical geometry—but by the abundance and ability of his labors in every department of science and philosophy, Descartes, for more than half a century, occupied a position in the learned world scarcely inferior to that which for ages preceding had been held by Aristotle.

As to the cause of planetary motion, Descartes assumed the existence, throughout the limits of our system, of a subtle transparent fluid in ceaseless revolution about the sun as its center, and that the planets floated in this fluid and were consequently carried round by the sun in its motion, just as in a whirlpool a cork or floating body is carried round by the motion of the water. To account for the difference in the times of revolution of different planets, he supposed that the velocity of the revolution of the fluid, at different distances from the sun was different. To account for the revolution of the satellites of the planets, he assumed that in the nighborhood of each planet this fluid revolved about the planet as a center. To this purely fanciful

hypothesis there are several fatal objections, as was subsequently demonstrated by D'Alembert, of which it will be sufficient to mention that the very existence of a spherical vortex is a mechanical impossibility. And yet such was the weight of the authority of its author, and the ingenuity with which it was defended by himself and his followers, that, as was mentioned above, it not only was received with general favor but for more than half a century it was accepted by most men of science without questioning and continued to be maintained by some, even after Newton had announced and demonstrated the law of gravitation. It is a notable illustration of the tenacity of error when once it becomes firmly fixed and widespread, that for some years after the publication of 'The Principia,' a Latin translation from the French of 'The Physics of Rohault'—a work entirely Cartesian—continued to be the text-book in Philosophy at the University of Cambridge—Newton himself being at the time Lucasian Professor of Mathematics. We have the authority of Playfair for the statement (which, indeed, has been called in question by Sir David Brewster, in his 'Life of Newton,' though so far as we have been able to see, without any sufficient reason) that the doctrines of 'The Principia' were introduced into the regular course of instruction at Cambridge by strategem. Dr. Samuel Clarke, a zealous advocate of the Newtonian Philosophy, prepared a new and more elegant translation of Rohault, with copious notes, in which the doctrines of 'The Principia' were explained and defended, and it was by this work, more directly than by the lectures of Newton himself, that Cartesianism was finally driven from the University.

Whilst Kepler's speculations as to the cause of the motions of heavenly bodies were soon supplanted by the hypothesis of Descartes, his more just views in regard to terrestrial gravity commended themselves to the scientific world and speedily passed into universal and abiding favor. In the memorable work of Galileo on the true system of the universe—completed the very year after Kepler's death, and published two years after; a work which, aside from its own merit, 'The Holy Inquisition,' by the persecution of its author, has made immortal—we find the doctrine of Kepler, on the subject of gravity, distinctly stated and elaborately defended. The Inquisition had power to imprison Galileo and commit copies of his work to the flames, but the truth it contained could not be burnt or bound. The earth 'still moved,' and matter continued to attract matter, unawed by the terrors of the Inquisition. The truth, once distinctly apprehended and announced, was never again to be lost, but was destined to grow in importance and be extended in its application far beyond the conceptions even of the great prophets of nature who were the first to proclaim it. The doctrine of Kepler on the subject of gravity may be regarded as,

historically, the foundation of that sublime superstructure which in a subsequent age was reared by Newton, and which, by reason of the magnitude of its proportions and the multiplicity of its details, all pervaded and determined by the most admirable unity, now stands and in all probability will ever stand, as the most imposing monument ever erected by the human intellect.

Although Kepler's theory, that bodies terrestrial mutually attracted each other, met with ready reception, more than thirty years elapsed after the publication of this work before the idea was entertained, at least favorably, of accounting for the revolutions of the heavenly bodies on the theory of the universality of the attraction of gravitation. Kepler indeed, as we have remarked above, alludes to such an hypothesis only however to expose, as he imagined, its fallacy. The motions of the heavenly bodies being curvilinear, whilst the motions of bodies under the influence of gravity were rectilinear, it was taken for granted as a thing self-evident that the two phenomena must be due to entirely different physical causes. Familiar as we are with the fact, that by the two laws of motion above mentioned, the hypothesis of an attractive force of the sun, combined with the hypothesis of a tendency of the planets to move in a straight line in virtue of an original impulse communicated to them, would satisfactorily and readily account for their curvilinear motion, it cannot but be a matter of surprise that the truth should have remained so long unrecognized.

The credit of having been the first to generalize the idea of gravity, and refer the revolutions of the heavenly bodies to the attraction of matter for matter, appears to be due to Borelli, an Italian philosopher, a pupil of Galileo. It is announced in a work which he published 'On the Satellites of Jupiter,' in 1666, although, as we shall have occasion to notice subsequently, Newton had conceived the same idea at least as early as 1665. Both Newton and Huyghens, however, attributed to Borelli the honor of having been the first to announce the important truth.

The idea, having been suggested, was at once accepted by many with favor and immediately led to the investigation of a hitherto unexplored field in the department of mechanical philosophy. Whilst the labors of others in this field were not unimportant, particularly those of Wallis, the name which is especially deserving of honorable mention in this connection is that of Huyghens. In a work published in 1672, we meet for the first time with a scientific discussion of *the doctrine of Central Forces.* His investigations were remarkably satisfactory and complete as to the phenomena of *circular* motion, the attractive force being at the center and contributing largely to the success of the labors of subsequent inquirers.

A great step had been taken toward the solution of the problem of planetary motion, but a formidable difficulty yet remained to be overcome. The orbits of the planets were not circular, but elliptical, and the sun—the center of the attractive force—was not at the center of the ellipse, but at one of the foci. For the complete solution of the actual problem which the phenomena presented, a calculus was needed which neither Borelli nor Huyghens possessed, and the preeminent genius of Newton was illustrated, probably more by the invention of the needed calculus than by his successful application of it to the solution of the important problem in question.

The general fact having been established that the curvilinear motion of the heavenly bodies was explicable on the hypothesis of a central attractive force, it was soon surmised that the particular character of the planetary orbits—involving as it did a continual variation in the distance of each planet from the sun, as well as a continual variation in the velocity of the planet's motion—could be due to no other cause than a difference in the intensity of the sun's attractive force at different distances. The query was: What was the precise law of this variation in intensity, which would account for the phenomena? Was the attraction inversely as the distance? or, as the square of the distance? or, as the cube? or, was it such as admitted of any precise expression? Guided probably by the best known fact as to the distribution of light, of heat, indeed of any emanation radiating in all directions from a center, several individuals, independently as it would seem, adopted the conclusion which was afterwards demonstrated to be correct, namely: That the attractive force of matter for matter varied inversely as the *square* of the distance, that is, at double the distance the attraction is one-fourth, at treble the distance one-ninth, and so on. The first to announce the true law of variation in the intensity of attraction was a French philosopher, Bouilland, or as his name ordinarily appears in the Latinized form, Bullialdus. About the same time, Sir Christopher Wren, the distinguished architect of St. Paul's, Dr. Hooke, for a long time secretary of the Royal Society, and the eminent mathematician astronomer, Halley, had arrived at the same conclusion. It was still however but a conjecture. In spite of the most earnest and persevering effort no one was able to furnish a demonstration.

As contributing to the discovery of the demonstration, the place of merit next to that of Newton, though of course far inferior, is doubtless due to Hooke. His labors were probably of aid to Newton by way of suggestion, without however affording any just ground for the charge which Hooke subsequently made that Newton was wearing the laurels to which he himself was justly entitled. As early as 1666 Hooke exhibited in the presence of the Royal Society an experiment

now quite familiar but at this time new and of exceeding interest. He suspended from the ceiling a long wire to the end of which a ball of wood was attached—a simple pendulum on a large scale. On removing the pendulum from the vertical position and then giving it a lateral impulse at right angles to the plane in which it tended to oscillate, the ball described an ellipse—the eccentricity of the ellipse varying with a variation of the intensity of the lateral impulse. An ocular demonstration was thus given of the important fact that elliptical motion could be produced by the combined action of two forces—one impulsive and the other central—and that the particular form of the ellipse depended upon the relative intensities of the two forces. Although in the experiment the attractive force was at the center of the ellipse, whilst in the case of planetary motion it was at one of the foci, still the fact exhibited must have been highly suggestive to any subsequent inquirer as to the cause of planetary motion.

In 1674 Hooke published a dissertation entitled 'An Attempt to prove the motion of the earth by observations,' in which he says: "I shall hereafter explain a system to the world, differing in many particulars from any yet known, depending upon three suppositions." The first—which he gave at some length—is a distinct statement of the universality of the attraction of gravitation. The second is substantially Kepler's law of inertia. The third is "that the attractive powers of the heavenly bodies are so much the more powerful, by how much nearer the body wrought upon is to their own centers." And, he adds, "Now what these several degrees are, I have not yet experimentally verified, but it is a notion which, if fully prosecuted, as it ought to be, will mightily assist the astronomers to reduce all celestial motions to a certain rule, which, I doubt not, will never be done without it." From this declaration it is evident, first, that at this time he was still in doubt as to the true law of gravitation; and second, that he was endeavoring to discover it by *experiment*—a method by which he could never have arrived at the truth. A few years later, as appears from his correspondence with Newton, Wren and Halley, he was fully convinced that the intensity of the attraction of gravitation was inversely as the square of the distance, and he even professed to be able to furnish a demonstration. In this he was either insincere at the time or discovered subsequently that his supposed demonstration was defective, as he never presented it, though repeatedly urged by Wren and Halley to do so.

We are now prepared to understand and appreciate aright the precise work which Newton performed in connection with the discovery of the law of gravitation. Born on Christmas day of the year 1642, the year in which Galileo died, in 1665 we find Newton a student of Trinity College, Cambridge, which he had entered in 1660. But

twenty-three years of age, he had already not only mastered all of value that had previously been written on Mathematics, Astronomy and Natural Philosophy, but he had discovered the Binomial Theorem, and had conceived and to an extent developed the Differential Calculus —an achievement with which few other events in the history of science deserve to be compared, after we except his own subsequent brilliant discoveries in Optics, and his successful application of the calculus to the discovery of the law and explanation of many of the most interesting phenomena of gravitation. In the summer of 1665 he left Cambridge on account of the plague which prevailed there at the time and returned to his native town of Woolsthorpe in Lancashire. It was during this visit to Woolsthorpe that the famous incident occurred which, as is generally supposed, first suggested to him the idea of gravitation and was the occasion of his great discovery. The account of it is given by his contemporary and friend Pemberton. One day as he was sitting under an apple tree in the garden an apple fell before him. This turned the currents of his thoughts and led him to reflect upon the nature of that mysterious influence which urges all terrestrial bodies toward the center of the earth, causing them, when free to move, to fall with a constantly accelerated velocity, which continues to act moreover without sensible diminution in intensity at the top of the highest towers or even the summit of the loftiest mountain. The thought was suggested to his mind, why may not this power extend to the moon? And if so, is not this the influence which retains her in her orbit round the earth? He at once applied himself to the determination if possible of the truth of this conjecture. If the moon were really retained in her orbit by terrestrial gravity, he concluded that the planets were probably retained in their orbits by a similar influence of the sun. Moreover, if the attractive influence of the earth extended to the moon and that of the sun to the farthest limits of our system, he concluded that the intensity of the attraction in each case diminished as the distance from the center of attraction increased. If this were so, it would manifest itself by a difference in the velocities of the planets, they being at different distances from the sun, and he accordingly inferred that by a comparison of the velocities of the motions of the several planets with each other, the law of variation of the intensity of the attractive force might be determined. Kepler's third law, that the squares of the times are as the cubes of the mean distances, furnished him at once with the necessary data for the calculation. He was not at the time able to solve the precise problem which the actual phenomena presented, the planetary orbits being elliptical and the attractive force at one of the foci, but assuming the orbits to be circular and the attractive force at the center, he found that Kepler's law would follow, if the variation in the

intensity of the attraction were *inversely as the square of the distance.*

It deserves to be noticed, that to solve even this problem, Newton must at the time have been familiar with the doctrine of central forces, though Huyghens' work on that subject was not published until more than six years after.

Though the data which Newton assumed were not precisely those which the planetary systems presented, the result reached was highly interesting and calculated to encourage and direct further inquiry. The next question to be determined was, the law of the variation of the *earth's* attraction—Was this also inversely as the square of the distance? If so, the universality of the attraction of gravitation varying in intensity according to the law just mentioned, would be almost indubitable.

The method by which Newton undertook to determine the variation of the earth's attractive influence—so simple when once suggested—was entirely original with him, and is one, though but one, of the grounds for attributing to him preeminently the honor of the discovery of the law of gravitation. Hooke, and doubtless others, subsequently labored for years to determine whether the intensity of the earth's attraction diminished with an increase of the distance from the center, and if so, according to what law, and yet all their efforts were fruitless. Newton's method was simply this, assuming the supposed distance of the moon from the earth to be correct, the length of the entire orbit of the moon may be readily determined. Moreover, the time of a complete revolution of the moon about the earth being known, the arc which she describes in one minute of time becomes known. Regarding this arc, which differs but little from a straight line, as the diagonal of a parallelogram, by the parallelogram of forces one of the sides of this parallelogram would represent the distance which the moon actually falls toward the earth under the influence of the earth's attraction in one minute of time. The arc just mentioned being known, this distance, which is the versed sine of the arc, may be readily determined. A measure is thus obtained of the intensity, at the moon, of the earth's attraction. By comparing this with the intensity of the attraction at the surface of the earth, as indicated by the distance a body near the surface will fall in one minute, the law of the variation in the intensity may be determined. Upon making the necessary computations the result was not just that which Newton anticipated, or rather hoped for. The distance which the moon ought to have fallen in one minute, according to the hypothesis, was one-sixth greater than that which, as it appeared, she actually did fall. Most men would have regarded this discrepancy as of little account, and accepting the result as, for the time at least, a sufficiently accurate demonstration of the hypothesis, would at once have given it publicity.

Newton, however, though he could not but feel assured that the true law of gravitation was indicated in the result he had reached, with that singular reticence as to his labors and indifference to fame which were among the marked features of his character, not only did not publish his investigations but did not even in his correspondence with his friends allude to the subject. For more than thirteen years he does not appear to have made any further progress toward the solution of the problem of gravitation. Though his attention was doubtless at times directed to it, he was mainly occupied during this period with other scientific labors, particularly in investigating the phenomena of light, making many brilliant discoveries on this subject which, even if he had not subsequently discovered the law of gravitation, would have entitled him to a distinction among men of science scarcely inferior to that which is now awarded him.

In 1679, after Bouilland, Hooke, Wren, Halley and others had become well convinced of the true law of gravitation and yet were unable to furnish a demonstration of it, Newton was led to a renewed investigation of the subject. Hooke had for some time been investigating the motion of projectiles, and in a letter to Newton about this time asserted that a body acted on by an impulsive force and at the same time by an attractive force varying in intensity inversely as the square of the distance, would describe an ellipse. What proof Hooke had of the fact asserted does not appear. It may be regarded as certain that he was not able to give a mathematical demonstration of it. As he had become well convinced that the attraction of gravitation varied according to the law mentioned, it is altogether probable that the main if not the sole ground for his assertion, was the fact that the orbits of the planets are elliptical. However this may be, Newton at once appreciated the importance of the assertion if it could be demonstrated, and was led to attempt the solution of the problem suggested by Hooke, or rather the converse problem, namely, to determine the law of variation in intensity of a central force which would cause the body acted upon to describe an ellipse. By the aid of the calculus, which he had by this time considerably perfected, he finally succeeded, after long and laborious effort, in demonstrating in its most general form the truth of Hooke's assertion. The importance of the result cannot be over estimated. The enigma which the elliptical orbits of the planets had presented was solved, and not only the fact of the sun's attraction but the precise law of the variation in intensity of that attraction was at last established beyond the possibility of further doubt or questioning.

The demonstration of the universality of gravitation however was still incomplete. The sun indeed attracted the planets with a force varying inversely as the square of the distance, but was this a property

common to all matter? Was it identical with the attraction of the earth which caused bodies near it to fall toward its center? Were the revolutions of the planets and the revolution of the moon phenomena referable to one and the same great law of nature? The result which Newton had reached in his investigations in 1665 seemed to render this doubtful, or at least presented a difficulty for the time inexplicable. Accordingly, with that characteristic reticence to which we have previously referred, Newton refrained from communicating to any one the important discovery he had made, preferring to await the solution of the difficulty which the anomalous fact of the apparent intensity of the earth's attraction at the moon presented.

Three years afterwards, in June, 1682, Newton attended a meeting of the Royal Society. Whilst in London, he accidentally learned that Picard in France had just measured the arc of the meridian with great accuracy, and that the result which he obtained for the length of a degree in that latitude differed somewhat from the measurement previously accepted as reliable. Newton at once perceived the importance of this fact in connection with the determination of the intensity of the earth's attraction. If the commonly received measure of a degree of the meridian was erroneous, the accepted estimate of the size of the earth was erroneous; moreover, if the assumed semi-diameter of the earth was incorrect, the supposed distance of the moon from the earth, in the calculation of which the earth's semi-diameter is involved, must also be incorrect. The possible explanation of the annoying result he had reached in 1665 was immediately suggested. Obtaining accurately the measurement of a degree of the meridian as given by Picard, immediately on his return to Cambridge he determined the size of the earth and the distance of the moon, on the supposition that Picard's measurement was the true one. With the data thus obtained he returned to the problem at which he had labored sixteen years before, and by the same method then pursued sought anew to determine the law of the variation of the earth's attraction. Perceiving, as he advanced in the calculation, the tendency of the numbers to produce the desired result, he became so much agitated that he was unable to finish the computation and was under the necessity of requesting a friend to do it for him. The identity of the force which causes bodies near the earth to fall toward its center and that which causes the heavenly bodies to revolve was fully established, the universality of the law of gravitation was finally and forever demonstrated, the solution of the grand problem of the universe was complete.

We might have supposed that Newton would have eagerly hastened to announce his great discovery and secure for himself the eminent honor to which he was entitled, and yet more than two years elapsed before the discovery was published to the world; and then, not of his

own motion but at the instance of his friend, Halley, who subsequently boasted that he was the Ulysses who had discovered Achilles and brought him forth from his concealment. In the month of August, 1684, Halley, having become satisfied that Hooke could not furnish the demonstration of the law of gravitation which he had repeatedly promised, visited Cambridge to confer with Newton on the subject on which he had become deeply interested and been long laboring without any satisfactory result. He inquired of Newton, what would be the curve described by the planets on the supposition that the attractive influence of the sun diminished as the square of the distance? Newton at once replied, 'An ellipse.' When asked how he knew this, he replied: 'I have calculated it.' Halley, surprised and delighted at the announcement, asked to see the demonstration. Newton was unable to lay his hands on the calculation he had made two years before, nor could he at the moment reproduce it. He promised Halley however that he would send him the demonstration as soon as he was able, and in the month of November following he fulfilled his promise. Halley immediately revisited Cambridge to obtain Newton's consent to the publication of the discovery. In this he succeeded, and on the 10th of December he informed the Royal Society of the discovery and that Newton had consented to prepare a paper on the subject for the society. In February, 1685, the promised communication was received—a paper of twenty-four pages, containing four theorems and seven problems. He refers to it in the accompanying letter, as his 'Notions about Motion.' This humble yet most memorable paper ever presented to the Society was the germ of 'The Principia.'

The great discovery having been made public, Newton seems to have felt that the time had come to enter on the gigantic task he had doubtless proposed to himself when the discovery was first made but which other occupations had hitherto prevented him undertaking, namely, putting his demonstration in a complete and conclusive form and applying it to the solution of the many interesting and sublime problems which the phenomena of the material universe presented. For two years he dismissed from his mind all other occupation, and devoted himself with all the energy of his mighty intellect to the Herculean task. With untiring industry, prolonged attention, and intense thought, probably never paralleled in the history of intellectual effort, he lived but to meditate and calculate; oftentimes so wholly absorbed with the grand themes which occupied his mind as to be for the time unconscious of all the ordinary concerns of life. Frequently, on rising in the morning he would sit for hours on his bedside arrested by some new conception, and had it not been for the attention of the members of his family would often have neglected to take his daily food.

We cannot enter upon any detail or present even a summary of the magnificent result of these labors. They are to be found in his immortal work, the 'Philosophiæ Naturalis Principia Mathematica,' given to the world in 1687 under the auspices of the Royal Society. Of this work, the great Laplace, who, of those who have applied the highest powers of the human mind to the investigation of the phenomena of gravitation, stands second only because Newton lived before him, says: "The universality and generality of the discoveries it contains, the number of profound and original views respecting the system of the universe it presents, and all presented with so much elegance, will insure to it a lasting preeminence over all other productions of the human mind." "It is a work," says Sir David Brewster, "which will be memorable, not in the annals of one science or one country only, but which will form an epoch in the history of the world, and will ever be regarded as the brightest page in the records of human reason. It is a work which would be read with delight in every planet of our system, and in every system in the universe. There was but one earth on whose form and movements and tides the philosopher could exercise his genius; one moon whose perturbations and inequalities and action he could study; one sun whose controlling force and apparent motions he could calculate and determine; one system of comets whose eccentric paths he could explore and rectify; one universe of stars to whose binary and multiple combinations he could extend the law of gravity. To have been the chosen sage, summoned to the study of that earth, these systems and that universe, the favored lawgiver to worlds unnumbered, the high-priest in the temple of boundless space, was a privilege that could be granted to but one member of the human family; and to have executed the task, was an achievement which, in its magnitude, can be measured only by the infinite in space, and in the duration of its triumphs by the infinite in time."

PLANTS AS WATER-CARRIERS.

By Professor BYRON D. HALSTED,

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A GIANT redwood, the monarch of the California forests, stands with its stem-tip three hundred and fifty feet above the soil. From the surface of the millions of tender delicate leaves near the top of the tree there are exhaled many gallons, perhaps barrels, of water daily. The force required to make good this loss is, of course, equal to that needed to raise the water through the three hundred feet or more of vertical space. It is no wonder that the thoughtful person will pause as he contemplates this exhibition of force. It makes no noise; work is being done, but it is not easy to see how.

Let us begin with the soil, as that is the source of the water supply of plants, and briefly consider its constitution, texture and relations to the problem of water-carrying. In other words, does soil carry water and, if so, in what way is it conveyed? Soil is rock that has been broken into small pieces in one way or other, a refinement of rock, so to speak, whether by frost, moving water or chemical action. For our purpose soils, having many degrees of fineness, may be classified into coarse, medium and fine. Coarse soil may be compared to masses of cannon balls touching each other but with large spaces between them, while the medium soil is similar to peas in piles, and the fine soil is like clover seed. The chief difference is in the amount of surface exposed by the particles which go to make up a definite portion of soil.

The next point for us to consider is the capacity of soil for holding moisture. Thrust the hand into a dish of water and upon removal it will be wet, except any portion that has been coated with oil or similar substance. In short, water will leave its own mass and adhere to the surface of the hand. If the hand held a quantity of clean earth the latter would likewise become wet. The amount of water that the soil will hold depends upon the surface exposure of its particles. As this is an exceedingly important point, permit me, at the risk of dealing largely in dry figures but for explanation and proof, to draw upon some results given by Professor King in his charming book 'The Soil.' With columns of sand ten feet long, the one with the grains averaging in diameter 186/10,000 inch, after percolating for 111 days, contained 3.77 per cent. of water; the 73/10,000-inch grains retained 4.92 per cent.; the 61/10,000-inch grains, 5.76 per cent. and the 45/10,000-inch grains, 7.57 per cent. In other words, the smaller the size of the

grains and the pores the greater the amount of water retained. With soils of still smaller particles the water-holding power would be correspondingly greater.

If the soils are placed in long upright glass tubes and water is added at the bottom, it will rise through the soil to the top. This phenomenon of capillarity is best illustrated with tubes having bores of varying diameter. The tables given us on this subject are to the effect that, when an inch tube is plunged into a vessel of water, the height of the water column in the tube above the general level is .054 inch; for a 1/10-inch bore .545 inch, 1/100-inch bore 5.456 inches and for a 1/1,000-inch bore, 54.56 inches. While the actual surface pull of the smallest tube is much less than that of the largest, it is through a vastly greater distance and by a multiplication of the number of such minute tubes in a given space that the greater lifting is brought about.

The soil itself, consisting of minute particles, admits of the capillary action; for the pores, although not straight, extend in irregular lines and permit the surface tension that is evident in fine tubes. This lifting power of minute passageways is abundantly illustrated in the everyday operations of crop-growing, and the skilful tiller makes abundant use of it or checks it as best suits his purpose. If a dark soil contains an abundance of light alkaline salt, it is possible that it may have a white crust form upon the surface during a drought to be carried back upon the falling of a substantial rain, and this rise and fall may be repeated indefinitely as happens on some of our alkaline lands, where the precipitation is light and vegetation scant.

It has been shown that the soil, on account of its porosity, is able to lift water through considerable distances, simply through the greater pull of a solid for the liquid than the liquid has for its own particles. The hand is wet by the water; a towel hung high with barely one corner dipping into the basin may become wet throughout, and, by evaporation, the dish may be pumped dry.

Into this complex physical porous mixture, to the component particles of which a liquid adheres with such force as to be present when even the air is dry, the plants establish themselves by means of their tiny rootlets and the much more minute root hairs which, insinuating themselves between the microscopic pebbles, become misshapen and contorted beyond all recognition of the simple vegetable cells out of which they have grown. The movements of the water in the soil, whether to the right or left, up or down, are governed, as has been shown, by the law of surface attraction. When we come to the plant cell, the whole physical basis is changed, and, among other things, we are brought face to face with membranes of extreme thinness and delicacy, and, more than all, with the living protoplasmic film.

The two ends of the microscopic aqueous canals, in plants of the ordinary sort, are the active imbibing root-hairs, above mentioned, and the exposed surfaces of the aerial parts, chief of which are the leaves. The lower terminal is buried beneath the soil from which it receives its supply, and consists of a sac with a very thin and elastic wall, lined with a delicate film of living protoplasmic substance. We may imagine one of these cells many hundred times enlarged, its contents consisting of a thin syrup, but slightly more dense than the liquid in which it is immersed. As time passes, there is a flow inward of the less dense liquid and an increase of the wall tension of the cell. This tension might be observed by pricking the wall, when from the pinhole the liquid would spurt for some distance. The same pressure might aid in the passage of the liquid from the sac to the one next adjoining, and in that way a flow would be set up from the less to the more dense cell contents.

A homely and common illustration of this osmosis or membrane diffusion is seen in the action of sugar upon ripe strawberries, the sugar taking the thin juice from the cells and making a syrup that finally surrounds the berries. Place dried prunes in a dish of warm water, and a similar exchange is demonstrated, but in this case the flow is into the dry cell contents, and the prunes finally become plump. There has been a transportation of liquid in both these instances, and it has been from one cell to another through the whole necklace, so to say, of many beads, from the surface to the innermost cell or *vice versa,* as the case may be.

Let us ascend a tall tree, figuratively, and study microscopically the upper terminals of the lines of water-carriers. Here we find the leaves in great numbers, presenting, possibly, acres of actual visible surface to the drying influence of the almost constantly changing air. But if we note the exceedingly porous structure of a leaf, how one cell touches its fellows at but few points and the bulk of the space is intercellular, the actual surface exposed to the atmosphere is a hundred times more than the naked eye reveals. As with the soil terminal, so here the end of the transportation line divide up into a million parts. In the former, each is for the reception of liquid; in the latter, they are all places of unloading. The drying air sweeps over them, and something of their contents is vaporized and is gone from the plant. But this evaporation increases the density of the cell contents and were there no reserve the tissues would wither, dry up and become dead, as is the case when a branch is cut off or grass is mown in the meadow.

If we apply the law illustrated in the dried prunes, it will be seen that each surface cell in the loose pulp of the leaf is dependent upon the next below it, and that, in honoring the draft upon it, is making a physical call upon another, and so the line is established, like men

passing buckets at a fire, or tossing melons in the loading of a schooner for a northern market.

The whole story of water-carrying is not ended with the above. One of the most delicate of all plant mechanisms is that which is associated with the transportation of its liquids. The leaves and green surfaces generally are closely studded with minute structures, 100,000 or more to a square inch, that open or close as the emergencies of the case demand. They are vitalized and exceedingly sensitive valves, usually constructed of two crescent-shaped cells set in the skin and highly charged with protoplasm. These organs are influenced by sunlight and darkness, by heat and cold; in fact, their functioning calls forth the admiration of any careful student of the subject. The two guard cells are so hung that they become turgid when the leaf is well filled with water, and thus enlarge the opening to its full capacity for the passage of vapor-laden gases. As soon as these guard cells lose much water, they become less plump, and this brings about the closing of the pore. They are, therefore, valves of safety, and, as the other portions of the leaf are covered with a cuticle more or less impervious to gases, it is seen that the stomates are the organs that regulate the evaporation stream.

That the amount of water carried is very great scarcely needs to be emphasized. Note the rapidity with which grass wilts when cut for hay or the leaves upon a branch that has received any injury. If a melon vine with twelve leaves will carry a liter of water in a single day, as it has been known to do, what must be the vastness of the lift in a forest of a thousand acres upon a dry day when the leaves are fresh and most active!

That it needs to be great is seen from the requirements of the plant. The soil water is weak in all salts that a plant must acquire, and to take them in concentrated form would be as poison. The whole plan, therefore, is to carry large quantities of a dilute solution, and afterwards bring it to the required strength. In the evaporation there is a cooling obtained that may possibly save the plant from destruction.

We thus far have seen that an ordinary plant has its slender, delicate, insinuating root-hairs closely applied to the soil particles from which they imbibe the adhering moisture. It has further been shown that the opposite terminal of the waterways has also a vast number of delicate living cells exposed, not dangerously, to the drying action of the atmosphere. Between these two extremities is the body of the tree, the main roots and branches, and it is for us to determine through what parts the upward flow takes place. This admits of demonstration by the removal of certain portions and observing the effects. That it does not take place through the central or heart wood is to be expected, for the cells here are often all filled up with lignin and coloring

matter, and the way is blocked; the canal is filled with débris, so to say, and has become disused. Again, the old central wood frequently decays until there is only the outer ring of the later-formed wood remaining with the bark that covers it. That the bark is not the water-carrier may be shown by removing a ring of it and thus breaking the connection without interrupting the upward flow. That it does pass through the young wood may be shown by cutting this portion without harming materially the bark or the heart wood, when the leaves quickly wither and the tree may die. In short, the sap-wood is well named, as through it the soil-water mounts upward from the roots to the leaves.

In many plants, however, there is no well-developed ring of wood. Either the stem is too young to have one or its construction such that it never appears. However, the same kind of tissue is somewhere to be found in the stem, usually in strands or portions of tough threads, as in the corn-stalk, and through these the crude sap is transported. Some of these succulent stems are so transparent that they admit of experiments which demonstrate both the path and the rate of the upward flow. For example, a balsam stem may be cut and, while fresh, plunged into a harmless colored liquid, as that of some aniline dye. It is found that the woody bundles are the first to take the stain and that it mounts upward with a rate that is an index of the flow of sap and may be some feet in a single hour. Another test for the rate is found in the use of a harmless salt, easily detected in extremely minute quantities by the spectroscope. Let it be lithium nitrate, for example, and its rise discovered by making sections of the stem at different distances and burning small fragments.

But having determined the place of entrance, line of ascent and point of departure of the aqueous stream, it by no means follows that all the forces have been named that bring about the transfer. That living plants carry water and make it one of the chief labors of all their active days is beyond question, but physicists and physiologists, chemists and biologists are as one concerning the mystery that here exists. A grape-vine stump bleeding in early spring is a stumbling block for them all, and they fall back upon 'root pressure,' a term more convenient for covering much ignorance than for service as a full, well-rounded explanation of the phenomena in question. Membrane diffusion will account for much, capillary attraction helps considerably, and the differences of gas pressure within and without the cells, as in the tapped sugar maple in early spring, count for something; but back of all is a vital force that has not been reduced to a physical or chemical basis.

THE SOLUBLE FERMENTS OR ENZYMES.*

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IT has been said somewhat sententiously that the advance of science consists simply in a change of problems; we achieve progress when we substitute for one problem another at once more delicate and more precise. The recent history of the theory of alcoholic fermentation furnishes a conspicuous illustration of this aphorism. Liebig's ingenious conception concerning the breaking-down of the sugar molecule by the decomposing albuminous compounds in dead and dying yeast cells—his notion being that the sugar is toppled over, so to speak, by the mechanical shock of other falling molecules—was forced to yield to Pasteur's apparently clear demonstration of the part played under natural conditions by the living yeast cell. More recently the conception of the living cell as the essential feature of the process has been dethroned in its turn, and alcoholic fermentation is now shown to rest on the action of an 'unorganized,' 'lifeless,' or 'soluble' ferment or enzyme, secreted by the yeast plant. Further, the action of this enzyme and of other and more familiar 'unorganized' ferments has been brought into line with some of the most characteristic activities of the living cell, and many general life phenomena have been shown to be in reality phenomena of fermentation.

The consequent focusing of attention upon the enzymes, their nature, mode of action and chemical relations, has already been prolific in results of great biological interest. The science of experimental medicine, in particular, is discovering that many of the problems relating to immunity, to toxins, antitoxins and agglutinating substances are closely connected with the problems of fermentation and the enzymes.

So far as is known, the peculiar substances called enzymes are produced only by the living cells of animals and plants, although there are certain inorganic substances that so closely reproduce the essential qualities of enzyme action that they might almost be termed 'inorganic enzymes.' The enzymes obtained from the animal or plant cell have

* *The Soluble Ferments and Fermentation.* J. Reynolds Green. Cambridge University Press, 1899.
Masson, Paris, 1899.
Traité de microbiologie, II., diastases, toxines et venins. E. Duclaux.
Les enzymes et leurs applications. J. Effront. Carré et Naud, Paris, 1899.

never been isolated in a pure condition, and hence no definite knowledge of their chemical composition and constitution has yet been secured. In general, enzymes are precipitated by alcohol from the watery extract of plant or animal tissue, but such a precipitate contains the enzyme inextricably mingled with other chemical compounds, and all attempts to separate out a pure enzyme have thus far signally failed. The whole field of enzyme study is therefore at present beset with the same sort of difficulties that the science of bacteriology encountered before the days of 'pure cultures,' and it is doubtless true that effects that are at the present time ascribed to individual enzymes are in reality caused by mixtures of distinct varieties.

The different kinds of enzymes are at present chiefly distinguished through the differences in the changes that they produce in other substances. The enzymes that act upon starchy substances, for instance, may be conveniently grouped together; those that disintegrate albuminous compounds may be similarly treated, and so on.

Enzymes converting starchy substances into sugar. The longest known and perhaps most thoroughly studied enzyme is a representative of the group that transforms insoluble carbohydrate substances into soluble ones. *Amylase,* or *diastase,* is the well-known enzyme that accomplishes the conversion of the starch of the barley-grain into sugar in the process of malting. The action of the enzyme in this process appears to be quite elaborate since the complex starch molecule passes through several stages during its conversion into sugar, the hardly less complex substances known as 'erythrodextrins' and 'achroödextrins' being formed on the way. The theory has been advanced that the starch molecule breaks down by the taking on of successive molecules of water and by subsequent decompositions, sugar (maltose) being formed at each splitting, together with a dextrin of lower molecular weight. Duclaux, however, maintains the existence of two enzymes in malt, one, a liquefying or decoagulating enzyme to which he would restrict the name amylase, and which converts the insoluble starch into the soluble dextrins, and a second (dextrinase), which has a saccharifying power and converts the dextrins into sugar. Other enzymes that may be placed in the same group with amylase are *inulase* and *cytase.* Inulase converts into fruit sugar a reserve food-substance found in many plants and known as inulin. Inulin is allied to starch in its chemical composition, and probably breaks down by successive stages under the action of inulase just as starch does under the influence of amylase. Another form of reserve food-substance stored up by many plants is the familiar substance comprising the cell-wall of most plants and known as cellulose. This substance, like inulin and starch, can be changed into a more directly utilizable substance by the action of *cytase,* an enzyme found

in many plant cells. The products of the activity of cytase have as yet been imperfectly studied, and it is possible that several different enzymes, corresponding to the different kinds of cellulose, are at present included under this name. Cytase may be useful in various ways to the organisms secreting it. Some parasitic fungi are able to attack and penetrate the cell-walls of the host-plant by virtue of the dissolving power of the cytase secreted in the tip of the growing hyphæ.

Enzymes acting upon sugars. Another group of enzymes is concerned with the conversion of the higher sugars or polysaccharides into the lower. The classic instance is the so-called 'inversion' of cane-sugar or sucrose into equal parts of grape-sugar and fruit-sugar, according to the equation:

$$\underset{\substack{\text{sucrose or}\\ \text{cane-sugar.}}}{C_{12}H_{22}O_{11}} + H_2O = \underset{\substack{\text{dextrose or}\\ \text{grape-sugar.}}}{C_6H_{12}O_6} + \underset{\substack{\text{levulose or}\\ \text{fruit-sugar.}}}{C_6H_{12}O_6}$$

A solution of cane-sugar turns a ray of polarized light to the right, but the mixture of dextrose and levulose, owing to the superior lævorotatory power of levulose, turns the ray to the left, whence the term 'inversion' as applied to this process. The enzyme that is able to produce the inversion of cane-sugar was discovered by Berthelot in 1860. *Sucrase,* or *invertase,* is found in many yeasts and other fungi, in pollen-grains, in the beet-root, and to a slight extent in some animal secretions. Cane-sugar is not directly assimilable by the animal body, and if injected into a vein is excreted almost unchanged. The inversion which occurs in the intestine when the cane-sugar is taken into the body by way of the alimentary tract seems to be a necessary preliminary to the utilization of this sugar as a food substance.

The same is true to a certain extent of maltose, which is a sugar of the same percentage composition as sucrose, but with a different arrangement of the atoms within the molecule. Maltose is split up by the action of the enzyme *maltase* into two molecules of dextrose. Maltose, like sucrose, is only with difficulty assimilable, and its conversion into dextrose constitutes an important phase of carbohydrate nutrition. Maltase, like sucrase, is a widely distributed enzyme and is found in many animal and plant tissues.

The action of maltase upon maltose presents a significant example of what chemists call a 'balanced' action. When a certain proportion of maltose has been converted into dextrose, the action of the maltase ceases, and if now an excess of dextrose be added the action of the enzyme is reversed, and a certain proportion of the dextrose is converted back into maltose until a new equilibrium be reached. This reversibility of action has been thought to indicate that the action of maltase falls in line with other chemical reactions and is not essentially different from that evinced by many well-studied inorganic substances.

Together with sucrase and maltase may be grouped other enzymes that split up the higher sugars into those of lower molecular weight, such as *lactase* which converts lactose or milk-sugar into dextrose and galactose, *trehalase* which splits up trehalose, a sugar obtained from Syrian manna, into two molecules of dextrose, and *raffinase* and *melizitase* which act upon certain of the higher polysaccharides.

Coagulating enzymes. The group of clotting or coagulating enzymes includes two comparatively well-known enzymes, *rennet* and *plasmase* or fibrin ferment. The use of rennet in setting curd for cheese and in preparing the delicate dessert known as junket is generally familiar. The source of most of the commercial rennet is the extract of the mucous membrane of the stomach of the calf; this enzyme is found also in many other young mammals during the period of lactation. Rennet has been obtained likewise from several vegetable sources; parts of the plant *Galium* are used in the country districts of England to aid in the formation of curd in cheese-making, and the peasants of the Italian Alps use the leaves of the butterwort (*Pinguicula*) for a similar purpose. The curdling or precipitation of the casein by rennet is singularly dependent upon the presence of salts of lime. A very minute quantity of rennet in the presence of calcium salts will curdle a prodigious quantity of casein. It is in fact uncertain whether rennet can act at all in the entire absence of calcium. In the presence of calcium the potency of rennet ranks higher than that of any other enzyme yet studied, one part of rennet being able to coagulate more than 250,000 times its own weight of casein.

The phenomenon of the clotting of blood is dependent upon a variety of factors as yet imperfectly understood. That the fibrin or solid portion of the clot is separated out from the blood plasma by the action of an enzyme is, however, solidly established. The character and mode of action of this enzyme—termed *plasmase,* or fibrin ferment—are still quite obscure, although the fact that in mammalian blood the enzyme originates from the leucocytes, or white blood corpuscles, seems to be generally admitted. In birds the enzyme exists in the cells of the tissues and not in the blood corpuscles. The blood of all vertebrates with nucleated red corpuscles presents a marked resistance to spontaneous coagulation; clotting, on the contrary, is almost immediate among the mammals, which possess enucleated red corpuscles. As is the case with rennet, calcium salts favor coagulation; their presence seems, however, not to be necessary.

Other clotting phenomena have been shown to be due to enzyme action. The formation of jelly from the juices of various fruits and berries is due to the gelatinizing or coagulating effect of an enzyme, *pectase,* which acts upon pectose, a carbohydrate allied to cellulose and occurring in many fruits and vegetables.

The various phenomena of clotting and gelatinizing belong in the debatable border land between physics and chemistry. Duclaux has given in his treatise a clear exposition of his reasons for believing that the changes involved in the various processes of coagulation are due to a disturbance of the physical equilibrium of the substances in solution rather than to any chemical reaction.

Enzymes acting upon proteid substances. One of the most important groups of enzymes is that of the proteolytic enzymes, characterized by their property of breaking down albuminous or proteid compounds into simpler ones. Owing to the prominent rôle they play in the human body in connection with digestive processes they have been subjected to exhaustive study. Two chief groups are recognized, the *peptic enzymes,* of which pepsin, the enzyme of the gastric juice, is the type, and the *tryptic enzymes,* the best known of which is the trypsin secreted by the mammalian pancreas. The peptic enzymes are almost unique among known enzymes, inasmuch as they can act only in an acid medium; they are further characterized by their inability to carry the decomposition of proteid substances beyond the 'peptone' stage. The tryptic enzymes, on the other hand, are most potent in a slightly alkaline medium, and they are able to push proteid decomposition to a point beyond that reached by the pentic enzymes. Two of the most characteristic end-products of tryptic digestion are the substances leucin and tryosin which, like urea, are not assimilable by the tissues and are eliminated from the body. Several tryptic enzymes of vegetable origin are known, among which *bromelin* from the juice of the pineapple and *papain* from the fruit of the papaw-tree have been thoroughly studied. It is probable that the digestive enzyme secreted by insectivorous plants belongs to the tryptic class.

An interesting tryptic enzyme has been recently discovered in fresh milk by Professors Babcock and Russell. This enzyme, called *galactase* by its discoverers, acts upon the proteids in milk and plays a most important part in the manufacture of cheese; it is probably responsible for many of the phenomena of cheese-ripening that were formerly ascribed to bacteria.

Owing to the great chemical complexity of proteid substances and to the fact that little is known about their chemical constitution, the study of proteolytic enzymes is hampered by difficulties of an especially serious nature. Since neither the initial composition of the proteid compound nor the substances to which it gives rise on decomposition can be accurately determined, the distinction of different kinds of proteolytic enzymes is attended with greater difficulty than is experienced in the case of enzymes that attack chemical compounds so comparatively well understood as the sugars.

The alcoholic enzyme. The enzyme that converts sugar into alcohol and carbon dioxide has been only recently discovered, although it has been diligently sought for since the time of Pasteur. The discovery of Buchner in 1896 that, by applying great pressure to a mass of yeast cells, an enzyme, which he named *zymase,* could be extracted gave in fact a new impulse to all enzyme study. Zymase appears to dislocate the sugar molecule according to the classic formula:

$$\underset{\substack{\text{dextrose}\\ \text{180 gr.}}}{C_6H_{12}O_6} = \underset{\substack{\text{alcohol}\\ \text{92 gr.}}}{2C_2H_5OH} + \underset{\substack{\text{carbon dioxide}\\ \text{88 gr.}}}{2CO_2}$$

The explanation of the prolonged failure of investigators to discover zymase lies in the fact that this enzyme is closely associated with the substance of the living yeast cell and does not diffuse out into the surrounding medium as does another common yeast enzyme already mentioned under the name of invert-ferment or sucrase. In solution, zymase quickly loses its strength, probably partly because of oxidation, partly because of the destructive action of the tryptic enzymes of yeast. Zymase is able to convert a number of different sugars into alcohol and carbon dioxide: maltose and sucrose are readily fermentable, galactose much less readily and lactose not at all. Glycogen can be slowly fermented by zymase, but is not fermented by the living yeast cell because it can not pass through the cell-membrane into the cell and zymase can not pass out. The brilliant researches of Emil Fischer upon the relation of the configuration of the sugar molecule to its fermentability have demonstrated how delicate is the relation obtaining between the structure of the sugar molecule and the enzyme that attacks it. A slight rearrangement in the position of the atoms within the molecule, the actual number of atoms remaining all the while the same, is sufficient to determine whether a sugar can be fermented or not. Only in those cases where the geometrical build of the enzyme conforms to that of the sugar molecule can fermentation occur. To use Fischer's metaphor, the enzyme must fit the substance it attacks as closely as the right key fits the wards of the lock that it opens.

Other enzymes. A few other important enzymes can be but briefly mentioned, since the limits of this review do not permit of a fuller consideration. A group of enzymes of which *emulsin* is the type may be classed as the glucoside-splitting enzymes. These ferments are able to split up glucosides—which may be described as compounds of glucose (or some other sugar) with an alcohol, ether, aldehyde, or similar body—into glucose and the aldehyde or other associated compound. *Emulsin* is found in many plant tissues, but it is doubtful if it occurs in any animal body. The physiological rôle of emulsin is not wholly understood; it is possible that the glucose formed by the enzyme action is useful in the nutrition of the plant, or it may be true that the toxic

or bitter principle also split off has a protective value and prevents injury to the plant by animals. Besides emulsin, a few other glucoside-splitting enzymes are known.

Lipase, a fat-splitting enzyme; *laccase,* an oxidizing enzyme concerned in the production of the famous black varnish used in lacquer work; and *urease,* an enzyme that converts urea into ammonium carbonate, are among the other better-known enzymes.

There is reason to believe also that the various anti-microbic substances found in the bodies of some artificially immunized and some naturally immune animals are to be regarded as enzymes, as is likewise the substance that is found in the blood of typhoid patients and that has a 'clumping' or 'agglutinating' action upon the typhoid bacilli. Research in this direction has not, however, proceeded far enough to enable us to offer anything more than a conjecture as to the real character of the 'agglutinines' and 'lysines.'

The precise mode of action of enzymes has been the theme of much speculation. Perhaps the simplest and most natural view of some cases is to suppose that the enzyme combines first, for example, with a molecule of water, and then attaches itself to the body upon which it acts. This new compound, meeting with another molecule of the same substance, is then decomposed into the body which the enzyme produces and the enzyme itself. The enzyme thus acts as a simple intermediary, bringing the molecule of water or oxygen in closer contact with the fermentable substance. This view has certain arguments in its favor, as for instance the fact that the enzyme does not exhaust itself in the course of the changes that it produces. If it is unceasingly decomposed and reconstituted the reason for this is clear.

Such an explanation, however, is hardly valid for the action of zymase upon sugar and for the reversible action of maltase. Now there are certain facts regarding the action of mineral acids upon sugars and proteids, of various salts upon the phenomena of clotting and oxidation and of other changes brought about by inorganic substances which render it difficult to set enzyme action apart as a thing by itself. The action of an enzyme is essentially 'catalytic,' that is, it is able to exert an influence wholly out of proportion to its quantity, and itself remain unaltered at the end of the process. It has been pointed out that the influence of an enzyme or indeed any catalytic agent is simply to retard or accelerate changes which ordinarily take place more slowly or more rapidly. In other words, an enzyme simply influences the rate of change, not the final condition of the substance upon which it acts. The nature of the change, the final state of chemical equilibrium, is determined by the chemical forces within the substance itself, the speed at which the change occurs is determined by the enzyme.

DISCUSSION AND CORRESPONDENCE.

GEOLOGY AND THE DELUGE AGAIN.

To the Editor:—The August number of The Popular Science Monthly contains a letter signed 'X. Plain' which asks Prof. G. Frederick Wright certain questions in regard to his recent article in 'McClure's Magazine' entitled 'Geology and the Deluge.' Is this quite a fair procedure on the part of X. Plain? He knows that Prof. Wright holds the chair of the harmony of science and religion in Oberlin College, and that he is filling the position in a manner satisfactory to his constituents. Then is it fair, I ask, for X. Plain, whoever he may be, to stand behind the door and, thus protected by an assumed name, take Professor Wright to task for doing his legitimate business in an acceptable, even masterly manner? X. Plain evidently does not dare to say what he thinks upon biblical matters. Nobody in this country does. Ingersoll said a few things, and I have heard good Christians say that he should have been burnt alive for saying them. Other very good Christians whom I know will not permit his works in their homes. Elbert Hubbard rather puts a damper upon the supposed harmony of science and biblical history by saying a few things every little while. Ingersoll was not afraid, and Hubbard is not afraid, but the rest of us are, X. Plain included. We do not dare to put in words our estimate of the Bible if we wish to retain our positions, either professionally or socially. The professor of biology to-day must teach evolution. The tide of evidence is so overpowering that he is carried *nolens volens* along with it. The church has been forced to accept the 'theory' of evolution. It has 'harmonized' apparent discrepancies, and has comforted trembling souls as mothers do their little ones: "There, there, there, Evolution shall not hurt you." But a professor of any one of the natural sciences is obliged to be a very juggler with words, in order to teach the truths of evolution to those who are able to comprehend them, and at the same time not disturb the faith of those who wish to keep intact the religion, or rather theology, which they have inherited. Because of that imperative law of evolution, self-preservation, we must earn our bread and butter. Therefore the myths of Genesis are 'reconciled' with science. Hence we fall meekly into line, and, as we dare not express our thoughts freely, we maintain a careful silence. In melancholy proof of which, I too must use the shield of anonymity.

Y. Obscure.

SCIENTIFIC LITERATURE.

ANTARCTICA.

The great national antarctic expeditions of Great Britain and Germany are now on their way to the southern hemisphere, the *Discovery* having set sail from Cowes on August 6, and the *Gauss* from Kiel on August 11. Those who wish to follow intelligently the results of the explorations of the next three years will want to know what has been already accomplished, and there are fortunately two books giving the necessary information. A little while since, The Macmillan Company published in America a translation by Mr. A. Sonnenschein of Dr. Karl Fricker's excellent work on the 'Antarctic Regions.' With true German thoroughness, he begins with the conjectures of Aristotle and follows the history of discovery to the recent expeditions. The book is elaborately illustrated with maps and photographs. Still more important is the 'Antarctic Manual' prepared for the use of the British expedition. Under the auspices of the Royal Geographical Society and through the initiative of Sir Clements Markham, Dr. George Murray, the director of the civilian scientific staff, has compiled a volume of 600 pages, giving information likely to be of use in the conduct of the expedition. The contributors are the most eminent British men of science. Lord Kelvin writes on atmospheric electricity and Professor Schuster on the aurora; Professor Darwin on tidal observations and Professor Glazebrook on the pendulum. The contributors on the natural sciences include Professors Bonney and Gregory, and Messrs. Lydekker, Boulenger, Fletcher and Murray. Then a number of older articles are reprinted, the 'Narrative' of Charles Wilkes, the 'Journal' of Dumont d'Urville, etc. The work has been prepared for the officers of the expedition, but a limited edition will be sold to the public through Mr. John Murray.

INVENTIONS.

'Twentieth Century Inventions; A Forecast,' by George Sunderland (Longmans, Green & Company) is a readable, sober and profitable book which is entirely free from the exaggerations which mark most efforts in this direction. The author takes the ground that the germs of future inventions are already formed and that future progress is but the evolution of present tendencies. Thus he develops a new form of steam engine from the principles of the construction of the aneroid barometer, a new form of railroad from the rope cableway, and a new method of typesetting from the linotype machine. It is a curious fact that some of his proposed methods have not only been invented, but actually used in this country; for instance, the inclined movable staircase, electric motors for house elevators, electric heating, and the generation of power by wave action have already been shown to be possible if not feasible economically. The author is sound in his general ideas of evolution, but it may be possible that the century will witness inventions which he and we cannot imagine.

THE PROGRESS OF SCIENCE.

THE DENVER MEETING OF THE AMERICAN ASSOCIATION.

The chief scientific event of August is the annual meeting of the American Association for the Advancement of Science, and the present meeting is of more than usual significance. It is doubtless a mere coincidence that the fiftieth meeting of the Association and the first meeting of the twentieth century should be the first to be held in the western states. The meeting itself is, however, nearly as important an event for science in the west as was the original foundation of the Association for science in the east. It means that the scientific men of the western states have now become sufficiently numerous and influential to meet on terms of equality with those of the east. The development of scientific work in the central and western states during the past ten years has perhaps never been rivaled in the history of civilization. Of the twelve American universities having in their faculties the largest number of scientific men, seven are in this region—Chicago, California, Michigan, Minnesota, Wisconsin, Illinois and Stanford. Each of these universities has on its faculties twenty-five or more scientific men, apart from medicine and engineering, and other institutions—Nebraska, Kansas, Missouri, Iowa, Indiana, Texas, Washington and more—will soon be of the same rank. With a prejudice that is not unreasonable, we assume that the scientific intelligence of the country may be measured by the percentage of people that subscribe to this journal. Massachusetts would, by this criterion, stand first, but Colorado would have twice the intelligence of New Jersey, California nearly three times the intelligence of Pennsylvania and Arizona ten times the intelligence of Maryland. During the past ten years the population of the western half of the country has not increased appreciably more rapidly than that of the eastern half, but its educational and scientific development has been truly marvelous.

The meeting of the American Association at Denver, midway between Chicago and the Pacific coast, will be largely attended by those scientific men for whom it is the geographical center, and the excursion to Colorado is so attractive that the eastern states are certain to be well represented. The council holds a preliminary meeting on August 24, but the meeting really opens on the twenty-sixth. In the morning there is the usual formal welcome by the governor of the state, the mayor of the city and other officers, and the presidency is transferred by Professor Woodward, of Columbia, to Professor Minot, of Harvard. On Monday afternoon the addresses of the vice-presidents are delivered, and on Tuesday the retiring president gives his address, the subject being 'The Progress of Science.' During the week the Association meets in nine sections, and more or less closely affiliated with them are the meetings of nine special societies. The usual entertainments are offered by the citizens of Denver, and excursions of more than usual interest are planned to precede and follow the meeting. The geology, paleontology, flora, archeology and mining resources of the region are of peculiar interest to scientific men, and the scenic beauty of the state and of the surrounding states is known throughout the world.

The Association is this year particularly fortunate in its retiring and in its incoming presidents. Other eminent men have presided over the Association, but perhaps not before have they united scientific eminence with such great services to the Association and the organization of science in America. Those who have heard or read the presidential addresses which Professor Woodward gave last year before the American Mathematical Society and before the New York Academy of Sciences will look forward with great interest to the Denver address, which we hope to publish in the next issue of this journal. Professor Minot, the incoming president of the Association—whose portrait is given as frontispiece—is known here and abroad for his important contributions to embryology, physiology, animal morphology and zoology. As a boy Minot collected insects, and his earliest publications were on entomological topics. Graduating at the age of twenty from the Massachusetts Institute of Technology in 1872, he could at that time find in America no good opportunity to carry on advanced studies and consequently went abroad and spent three years in Germany and France. He was given the S.D. by Harvard in 1878 and appointed lecturer in the medical school in 1880, being promoted to an assistant professorship in 1887 and to the full professorship of histology and embryology in 1892. At first Minot's work was chiefly physiological—while a student under Ludwig at Leipzig he published an article showing that muscles can maintain their contraction without forming carbonic acid—and in the direction of experimental biology, his investigations covering topics such as growth, heredity and the differentiation of tissues. This work led to two important laws, namely, that, aside from minor fluctuations, the power of growth diminishes from birth onwards, there being really in animals no period of development as opposed to decline; and that the decline in the rate of growth is correlated with the increase and differentiation of the protoplasm of the cells. Another field of early study was the structure of worms. Here his most important result was the demonstration that the Nemertean worms, which had always been classed with the Plathelminths, form a distinct class. The microscopic anatomy of insects and vertebrates was the subject of a number of investigations, among them an extended essay on the histology of the locust, which contains many new observations on insect anatomy.

Owing to the claims of his professorship, strictly embryological work has steadily grown more predominant during the last twenty years. His first important embryological paper was a comparative study of the uterus and placenta, being the first comprehensive account of the microscopic anatomy of the human uterus during pregnancy, and containing many additions to knowledge. During recent years his writings—which in all number over one hundred and fifty titles—have chiefly presented the results of various embryological investigations. The book on 'Human Embryology,' first published in 1892, is a standard work here and in Europe.

As has been indicated, Professor Minot, while making these important contributions to science and conducting a department in a great medical school, has found time to take a leading part in what may be called the organization of science. He has written admirable articles and addresses of a general character, published in this and in other journals; he has by his publications and personal efforts done much to advance medical education and to unite it with biological research; he has accomplished much for bibliography, for the building and equipment of laboratories and in other directions. Following the suggestion of

Professor Hyatt, he founded the American Society of Naturalists and has been its president; he was one of the active founders of the Marine Biological Laboratory at Wood's Holl, and it was chiefly through his exertions that the American Society for Physical Research was started. He has been president of the American Morphological Society, and since 1897 president of the Boston Society of Natural History. He is, of course, a member of the National Academy and of many other scientific societies. In 1885 he was general secretary of the American Association and in 1890 vice-president for the section of biology. The Association is fortunate when the country produces for its presidents such men as Professors Woodward and Minot.

THE BRITISH CONGRESS ON TUBERCULOSIS.

The recent congress in London was as much an institution for public education as a scientific meeting. As a rule people do not profit greatly by learning about the diseases to which they are subject, but consumption is an exception. This disease is still regarded by many as hereditary and incurable; its existence is consequently ignored and concealed, and becomes a source of danger to others. But consumption is a curable and especially a preventable disease. Post-mortem examinations of those dying by accident show that about one-half of all people living in cities have had tuberculosis of the lungs, usually of course without knowing it. The disease has been cured without precautions and under unhygienic conditions. When detected in time, tuberculosis is not only curable, but is one of the most easily cured of chronic diseases. It has been decreased by one-half in Great Britain by improved sanitary conditions; it has within a few years been decreased by one-third in New York City as the result of municipal control. The disease is chiefly spread by the tubercle germs in the sputum carelessly scattered abroad, and chiefly favored by general insanitary conditions. It is consequently a matter of great concern, both to those who suffer from consumption and to those brought in contact with them,—and practically every one belongs to one of these clases—that the public should be educated to understand and support the measures required to combat the most terrible of all diseases.

If the congress in London accomplished more for the education of the laity than for the increase of knowledge, this is not to be regretted. The reception of foreign delegates, their presentation to the king and elaborate entertainment, led to the wide reporting of the proceedings in the press, and many of the papers were intended for the general public rather than for the specialist. Professor Koch's admirable address, which is published in this issue of the Monthly, can be read with interest and profit by any one, though it contains the announcement of important scientific research. Professor Koch's claim that the bovine tubercle cannot develop in the human body naturally attracted much attention, as it is obviously a matter of great practical importance. Lord Kelvin, Professor Virchow and other authorities, however, do not regard Professor Koch's experiments and observations as conclusive. Attention does not seem to have been called at the congress to the important experiments of Dr. Theobald Smith, of Harvard University, published some three years ago in 'The Journal of Experimental Medicine.' These demonstrate the difference between the human and bovine tubercle bacilli.

THE RECENT COMET.

The first brilliant comet of the century has come and gone. Although at one time so bright that it was visible in daylight, it was seen by few persons and at but two northern observatories. It was discovered, April 24, by Mr.

Halls, of Queenstown, Cape Colony, and was later, but independently, discovered at the Peruvian Station of the Harvard Observatory and elsewhere. Its path lay about 20° south of the sun, so that it was especially well situated for observers in the southern hemisphere. It seems remarkable that so bright a comet could escape more general attention. Bad weather and its southern position in part account for this, but the chief reason is associated with the path of the comet, and the position in which the earth chanced to be at the time. From the interstellar spaces the comet swept into the solar system on the opposite side of the sun from the earth. On this account, doubtless, it was not seen until it had already passed perihelion. At that time it was visible in the morning. A week later it was seen in the evening sky. At one time, except for the inclination of the plane of its orbit to that of the earth, it was moving directly towards us, but, swung about by the sun's attraction, it passed between that luminary and the earth. By the middle of May the comet and the earth were moving in nearly opposite directions. For a large part of the time during which the comet was under observation, it was visible only in strong twilight. About May 5 its position was more favorable, and it was a splendid object. It has now passed out of sight. The comet is described by Mr. Innis, of the Cape Observatory, when first seen, as of a deep yellow color. The nucleus was condensed, and of about the same brightness as Mercury. It had a tail about 10° long, but no coma, or 'hair.' As soon as the comet had emerged from the evening twilight, early in May, its most unique feature became apparent. This was a faint secondary tail, which preceded the comet, as it left the sun, at an angle of about 40° from the primary tail, which had become double. The main tail at this time, according to Mr. Lunt, was about 7° long, while the faint one was three times as long, or about 25°. Between these two were also two other very faint tails. At no time did the comet approach very near to the sun, or to the earth. Good photographs of it were obtained at the Royal Observatory, Cape of Good Hope, and at the Harvard Station in Peru. This adds one more brief but interesting chapter to the history of comets; but in spite of their frequent appearance and the attention which they receive, comets still remain, in many respects, one of the unsolved astronomical puzzels.

VITRIFIED SILICA.

One of the most promising of recent developments in connection with chemical and physical apparatus has been the discovery of practicable methods of working vitrified quartz. With all the serviceability of glass and porcelain, there is a real need for some plastic material, more infusible, more insoluble, more fully transparent, more elastic, and more stable under changes of temperature than glass. These needs would be supplied by quartz, were it not for the great difficulty of working it. When touched with the flame, quartz splinters so badly as to be almost unworkable, though in time past a few have used it for small objects, and some ten years ago Professor Boys introduced the use of quartz fibres, which have found several important applications in the physical laboratory. To Professor W. A. Shenstone, however, belongs the credit of having rendered practicable the working of quartz into more or less complicated apparatus. The most important step in his process is the preparation of a non-splintering silica, which he accomplishes by heating quartz in small pieces to a temperature of about 1,000°C. and then throwing it into cold water. The white, enamel-like mass obtained can then be subjected to any changes of temperature without splintering. It is worked in the hottest possible oxy-hydrogen

flame, becoming plastic enough for manipulation only above the melting point of platinum. In preparing tubes the first step is a rod, which is made by fusing small pieces of silica together, one after another. This rough rod is then re-heated and drawn out into finer rods about a millimeter in diameter. These rods are then bound around a thick platinum wire and heated till they adhere to each other forming a tube which can then be drawn out and worked much as a tube of glass. By blowing a small bulb on the end of the tube, surrounding it with a ring of silica, re-heating and blowing, the tube can be lengthened or the bulb enlarged at will. From this starting point it is possible to make quite complicated apparatus.

An examination of vitrified silica reveals several properties which give it a peculiar value for many purposes. Its melting point is so high that a platinum wire imbedded in a thick silica tube can be fused so as to flow out, before the tube softens sufficiently to lose its shape. Its coefficient of expansion is far less than that of any similar substance, being only one-seventeenth that of platinum. This expansion is very regular up to 1,000°, when it diminishes rapidly up to 1,200° and from this point on it contracts. Up to 1,500° it remains practically solid. In its expansion it differs very markedly from quartz, which not only has a much higher coefficient of expansion, but which at 570° expands so rapidly that it is shattered. A rod of vitrified silica can be heated white hot and then immediately plunged into liquid air without suffering injury, indeed it gains in elasticity when thus treated. These properties promise to be of great value in the construction of thermometers. Its transparency to the ultra-violet rays of the spectrum will give it a decided advantage over glass in spectroscopic work. It is also interesting to note that tubes of silica can be heated sufficiently high for nitrogen and oxygen to unite directly on passing through them. It is, on the other hand, slightly permeable to hydrogen at a temperature of 1,000°C. Altogether vitrified silica offers an interesting field of development in the immediate future.

TWO REMARKS CONCERNING THE 'MONTHLY.'

THE following paragraph is reproduced from the 'Electrical World,' as a favorable occasion for two remarks that it has for some time seemed desirable to make:

In the current number of our esteemed contemporary, the POPULAR SCIENCE MONTHLY, which is, alas! more popular than scientific in the single particular that its pages lie largely sealed from mortal eye until separated by that anachronous atrocity—the paperknife—appears a delightful article by Professor J. J. Thomson, 'On Bodies Smaller than Atoms,' an abstract of which appears in the Digest. It is a commentary upon the spread of technical education that a paper on this most abstruse subject, and actually containing some little algebra, should find the light of day in popularized literature, and awake a gleam of recognition from the eyes of many who are not scientists. Twenty years ago such an article on such a subject would have lain on popular benches as caviare to the multitude. It is difficult to say which commands our admiration the more—the article itself, or the fact that the great world should be capable of admitting it into semi-popular literature. Either consideration presents a triumph, the one over inanimate, the other animate, nature.

The first remark concerns trimming the pages of the MONTHLY. It appears from the correspondence of the publication department that to trim or not to trim is a burning question. An 'anachronous atrocity' is pretty strong language, but it seems to define a widespread creed. Some people apparently do not know that there is a good scientific reason for not trimming the edges, namely, that a magazine or book that has been trimmed cannot be properly bound afterwards. Conse-

quently all librarians prefer untrimmed copies, and the publishers of this magazine must provide for some fifteen hundred libraries. Then, we are not prepared to admit that it is unscientific to regard æsthetic considerations. Untrimmed copies look better to most people, and there are a few who even enjoy the use of the paperknife. This preference may be in large measure a survival; still a trimmed magazine seems to be ready for the waste-paper basket, whereas an uncut copy seems to be waiting for its place on the library shelf. Accordingly, copies of this magazine with the edges cut are supplied to the news-stands, but untrimmed copies are mailed to subscribers. Any subscriber, however, who asks for trimmed copies will receive them.

The second remark to which the editorial in the 'Electrical World' gives occasion is more important. It is indeed a matter for congratulation that this country is able to support a journal which calls itself popular and yet publishes only articles strictly scientific in character. Such a journal obviously does not appeal to children or to superficial readers; and its very existence bears witness to the presence in America of a large class of highly educated and thinking people. It may be, however, that some of those who have read the magazine for thirty years regret a certain change in its character and do not appreciate that this is simply an evolution fitting it to existing conditions. Some years ago the truths of evolution needed a fearless advocate, but when these are preached from the pulpit there is no longer need of a special organ. The daily press now publishes articles everywhere of a readable and light character on scientific topics, and no monthly magazine is complete without one or two such articles. What the country needs is a journal that will set a standard of accuracy and weight, and will separate the real advances of science from the vagaries of the charlatan. An article such as Professor Thomson's 'On Bodies Smaller than Atoms' must be read with care, but when understood, it is, as the 'Electrical World' remarks in the editorial from which we have quoted, 'more entertaining than the story of the early crusades and more astounding than those of the Arabian Nights.'

SCIENTIFIC ITEMS.

By the death of Charles Anthony Schott, the government loses one of its most distinguished officers. He was born in Germany, but was for fifty-three years connected with the U. S. Coast and Geodetic Survey. His distinguished position in the scientific world is sufficiently indicated by the fact that the Paris Academy of Sciences made to him the first award of the Wilde prize, which is given without regard to nationality for the most important researches in the physical sciences.—We regret also to record the death of H. W. Harkness, a student of the cryptogams and prominent for his services to science on the Pacific coast, having been for many years president of the California Academy of Sciences; of James Marvin, formerly professor of mathematics and astronomy and chancellor of the University of Kansas; of Williis H. Barris, known for his contributions to paleontology and long president of the Davenport Academy of Sciences; of George K. Lawton, an astronomer of the U. S. Naval Observatory, and of Charles Mohr, a well-known botanist, recently connected with the Geological Survey of Alabama.—Among foreign students of science the following deaths are announced: of Henri de Lacaze-Duthiers, the eminent French zoologist; W. Schur, professor of astronomy at Göttingen; Johannes Lamp, a geodesist of Kiel University; Henri d'Orléans, known for his geographical explorations in Asia and Africa; of C. E. Peek an English meteorologist;

Eleanor A. Ormerod, the English entomologist; and Baron Adolf Erik Nordenskjöld, the Swedish arctic explorer and naturalist.

Professor Rudolf Virchow will celebrate his eightieth birthday on October 13; a research fund is being collected in his honor and he has been made a knight of the Prussian order 'pour le mérite,' this mark of imperial favor having been long delayed, apparently owing to his liberal politics.—Professor A. W. Rücker, the physicist, has been elected principal of the newly organized London University.—Two of the prizes created by the will of Alfred Nobel will be awarded to Dr. Niels R. Finsen of Denmark, for discovering the light treatment for lupus, and to Professor I. P. Pavlov, the Russian physiologist, for his researches in nutrition.—Dr. Patrick Manson has been awarded the Stewart prize of the British Medical Association for his researches in pathology and tropical diseases.

Sir John Murray has returned from a six months' expedition to Christmas Island, during which he crossed the island from end to end, the first occasion on which it has been traversed.—Prof. Frederick W. Starr, of the University of Chicago, has completed a four months' expedition among the Mexican Indians.—Professor Engler, director of the Botanical Garden at Berlin, has visited the Canary Islands, in order to study their flora.—Professor C. L. Bristol, of New York University, has left New York to direct the Biological Station at Bermuda.

An international botanical association had its first meeting at Geneva beginning on August 7.—The Fifth International Congress of Criminal Anthropology will be held in Amsterdam from September 9 to 14, 1901.—The summer session of the American Mathematical Society was held at Cornell University, Ithaca, N. Y., during the week beginning on August 19.—The American Forestry Association will hold its meeting in affiliation with the American Association at Denver on August 27, 28 and 29.

According to the census taken on March 31, the population of England and Wales was 32,525,716, being an increase of 12.15 per cent. in ten years. The increase in the preceding decennium was 11.65. The percentage increase of London was only 7.3 per cent., its population now being 4,536,034. There has, however, been a large increase in the surrounding country, the population of Middlesex having nearly doubled. The population of Ireland is 4,456,546 and of Scotland 4,471,957. The change in the population of Ireland and of Scotland in the past sixty years is remarkable:

Year.	Ireland.	Scotland.
1841	8,197,000	2,620,000
1851	6,574,271	2,888,742
1861	5,798,967	3,062,294
1871	5,412,377	3,360,018
1881	5,174,836	3,735,573
1891	4,704,750	4,025,647
1901	4,456,546	4,471,957

THE POPULAR SCIENCE MONTHLY.

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THE PROGRESS OF SCIENCE.*

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A CONSTITUTIONAL provision of our Association stipulates that "It shall be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided." Happily for those of us who must in turn fulfill this duty the scientific foresight of our predecessors set no metes and bounds with respect to the subject-matter or the mode of treatment of the theme that might be chosen for such an address. So far, therefore, as constitutional requirements are concerned, a retiring president finds himself clothed for the time being with a degree of liberty which might be regarded as dangerous were it not for an unwritten rule that one may not hope to enjoy such liberty more than once. But time and place, nevertheless, as well as the painful personal limitations of any specialist, impose some rather formidable restrictions. One may not tax lightly, even, in a summer evening, the patience of his audience for more than an academic hour, the length of which in most cases is less than sixty minutes. One must confine himself to generalities, which, though scientifically hazardous, serve as a basis for semi-popular thought; and one must exclude technical details, which, though scientifically essential, tend only to obscure semi-popular presentation. Courtesy, also, to those who are at once our hosts and our guests requires that, so far as possible, one should substitute the vernacular for the 'jargon of science,' and draw his figures of speech chiefly from the

* Address of the retiring president of The American Association for the Advancement of Science, given at the Denver Meeting, August 27, 1901.

broad domain of every-day life rather than from the special, though rapidly widening, fields of scientific activity.

Between this nominally unlimited freedom on the one hand, and these actually narrow restrictions on the other, I have chosen to invite your attention for the hour to a summary view of the salient features of scientific progress, with special reference to its effects on the masses, rather than on the individuals, of mankind. We all know, at least in a general way, what such progress is. We are assured almost daily by the public press and by popular consent that the present is not only an age of scientific progress, but that it is preeminently the age of scientific progress. And with respect to the future of scientific achievement, the consensus of expert opinion is cheerfully hopeful, and the consensus of public opinion is extremely optimistic. Indeed, to borrow the language sometimes used by the rulers of nations, it may be said that the realm of science is now at peace with all foreign parts of the world, and in a state of the happiest domestic prosperity.

But times have not been always thus pleasant and promising for science. As we look backward over the history of scientific progress it is seen that our realm has been taxed often to the utmost in defense of its autonomy, and that the present state of domestic felicity, bordering on tranquillity, has been preceded often by states of domestic discord bordering on dissolution. And, as we look forward into the new century before us, we may well enquire whether science has vanquished its foreign enemies and settled its domestic disputes for good and all, or whether future conquests can be made only by a similarly wasteful outlay of energy to that which has accompanied the advances of the past. Especially may we fitly enquire on an occasion like the present what are the types of mind and the methods of procedure which make for the progress, and what are the types of mind and the methods of procedure which make for the regress, of science. And I venture to think that we may enquire also with profit, in some prominent instances, under what circumstances in the past science has waxed or waned, as the case may be, in its slow rise from the myths and mysticism of earlier eras to the law and order of the present day. For it is a maxim of common parlance, too well justified, alas! by experience, that history repeats itself; or, to state the fact less gently, that the blunders and errors of one age are repeated with little variation in the succeeding age. This maxim is strikingly illustrated by the history of science, and it has been especially deeply impressed upon us—burnt in, one might say—by the scientific events of our own times. Have we not learned, however, some lasting lessons in the hard school of experience, and may we not transmit to our successors along with the established facts and principles of science the almost equally well established ways and means for the

advancement of science? Will it be possible for society to repeat in the twentieth century the appalling intellectual blunders of the nineteenth century, or have we entered on a new era in which, whatever other obstacles are pending, we may expect man to stand notably less in his own light as regards science than ever before? To a consideration of these and allied questions I beg your indulgence, even though I may pass over ground well known to most of you, and encroach, perhaps, here and there, on prominences in fields controversial; for it is only by discussion and rediscussion of such questions that we come at last, even among ourselves in scientific societies, to the unity of opinion and the unity of purpose which lead from ideas to their fruitful applications.

From the earliest historic times certainly, if not from the dawn of primitive humanity, down to the present day, the problem of the universe has been the most attractive and the most illusive subject of the attention of thinking men. All systems of philosophy, religion and science are alike in having the solution of this problem for their ultimate object. Many such systems and sub-systems have arisen, flourished and vanished, only to be succeeded by others in the seemingly Sisyphean task. Gradually, however, in the lapse of ages there have accumulated some elements of knowledge which give inklings of partial solutions; though it would appear that the best current opinion of philosophy, religion and science would again agree in the conclusion that we are yet immeasurably distant from a complete solution. Almost equally attractive and interesting, and far more instructive, as it appears to me, in our own time, is the contemplation of the ways in which man has attacked this perennial riddle. It is, indeed, coming to be more and more important for science to know how primitive, barbarous and civilized man has visualized the conditions of, and reached his conclusions with respect to, this problem of the centuries; for it is only by means of a lively knowledge of the baseless hypotheses and the fruitless methods of our predecessors that we can hope to prevent history from repeating itself unfavorably.

Looking back over the interval of two to three thousand years that connects us by more or less authentic records with our distinguished ancestors, we are at once struck by the admirable confidence they had acquired in their ability to solve this grand problem. Not less admirable, also, for their ingenuity and for the earnestness with which they were advanced, are the hypotheses and arguments by which men satisfied themselves of the security of their tenets and theories. Roughly speaking, it would appear that the science of the universe received its initial impulse from earliest man in the hypothesis that the world is composed of two parts; the first and most important part being in fact,

if not always so held ostensibly, himself, and the other part being the aggregate of whatever else was left over. Though dimly perceived and of little account in its effects, this is, apparently, the working hypothesis of many men in the civilized society of to-day. But the magnitude of the latter part and its inexorable relations to man seem to have led him speedily to the adoption of the second hypothesis, namely, that the latter part, or world external to himself, is also the abode of sentient beings, some of a lower and some of a higher order than man; their rôle tending on the whole to make his sojourn on this planet tolerable and his exit from it creditable, while yet wielding at times a more or less despotic influence over him.

How the details of these hypotheses have been worked out is a matter of something like history for a few nationalities, and is a matter absorbing the attention of anthropologists, archeologists and ethnologists as it concerns races in general. Without going far afield in these profoundly interesting and instructive details, it may suffice for the present purposes to cite two facts which seem to furnish the key to a substantially correct interpretation of subsequent developments.

The first of these is that the early dualistic and antithetical visualization of the problem in question has persisted with wonderful tenacity down to the present day. The accessible and familiar was set over against the inaccessible and unfamiliar; or what we now call the natural, though intimately related to, was more or less opposed to the supernatural; the latter being, in fact, under the uncertain sway of, and the former subject to the arbitrary jurisdiction of, good and evil spirits.

The second fact is that man thus early devised for the investigation of this problem three distinct methods, which have likewise persisted with equal tenacity, though with varying fortunes, down to the present day. The first of these is what is known as the à priori method. It reasons from subjective postulates to objective results. It requires, in its purity, neither observation nor experiment on the external world. It often goes so far, indeed, as to adopt conclusions and leave the assignment of the reasons for them to a subsequent study. The second is known as the historico-critical method. It depends, in its purity, on tradition, history, direct human testimony and verbal congruity. It does not require an appeal to Nature except as manifested in man. It limits observation and experiment to human affairs. The third is the method of science. It begins, in its elements, with observation and experiment. Its early applications were limited mostly to material things. In its subsequent expansion it has gained a footing in nearly every field of thought. Its prime characteristic is the insistence on objective verification of its results.

All these methods have been used more or less by all thinking men. But for the purpose of ready classification it may be said that the first has been used chiefly by dogmatists, including especially the founders and advocates of all fixed creeds from the atheistic and the pantheistic to the theistic and the humanistic; the second has been used chiefly by humanists, including historians, publicists, jurists and men of letters; and the third has been used chiefly by scientists, including astronomers, mathematicians, physicists, naturalists, and more recently the group of investigators falling under the comprehensive head of anthropologists. The first and third methods are frequently found to be mutually antithetical, if not mutually exclusive. The second occupies middle ground. Together they are here set down in the order of their apparent early development and in the order of their popularly esteemed importance during all historic time previous to, if not including, this first year of the twentieth century.

No summary view of the progress of science, it seems to me, can be made intelligible except by a clear realization of these two facts, which may be briefly referred to as man's conception of the universe and his means of investigating it. What, then, in the light of these facts, has been the sequel? The full answer to this question is an old and a long story, now a matter of minute and exhaustive history as regards the past twenty centuries. I have no desire to recall the dramatic events involved in the rise of science from the Alexandrian epoch to the present day. All these events are trite enough to men of science. A mere reference to them is a sufficient suggestion of the existence of a family skeleton. But, setting aside the human element as much as possible, it may not be out of place or time to state what general conclusions appear to stand out plainly in that sequel. These are our tangible heritage and upon them we should fix our attention.

In the first place, the progress of science has been steadily opposed to, and as steadily opposed by, the adherents of man's primitive concepts of the universe. The domain of the natural has constantly widened and the domain of the supernatural has constantly narrowed. So far, at any rate, as evil spirits are concerned, they have been completely cast out from the realm of science. The arch fiend and the lesser princes of darkness are no longer useful even as an hypothesis. We have reached—if I may again use the cautious language of diplomacy—a satisfactory modus vivendi if we have not attained permanent peace in all our foreign relations. Enlightened man has come to see that his highest duty is to cooperate with Nature, that he may expect to get on very well if he heeds her advice, and that he may expect to fare very ill if he disregards it.

Secondly, it appears to have been demonstrated that neither the à

priori method of the dogmatists nor the historico-critical method of the humanists is alone adequate for the attainment of definite knowledge of either the internal or the external world, or of their relations to one another. In fact, it has been shown over and over again that man cannot trust his unaided senses even in the investigation of the simplest and most obvious material phenomena. There is an ever-present need of a correction for personal equation. Left to himself, the à priori reasoner weaves from the tangled skein of thought webs so well tied with logical knots that there is no escape for the imprisoned mind except by resort to the weapon applied to cobwebs. And in the serenity of his repose behind the fortress of 'liberal culture,' the reactionary humanist will prepare apologies for errors and patch up compromises between traditional beliefs and sound learning with such consummate literary skill that even 'the good demon of doubt' is almost persuaded that if knowledge did not come to an end long ago it will soon reach its limit. In short, we have learned, or ought to have learned, from ample experience, that in the search for definite, verifiable knowledge we should beware of the investigator whose equipment consists of a bundle of traditions and dogmas along with formal logic and a facile pen; for we may be sure that he will be more deeply concerned with the question of the safety than with the question of the soundness of scientific doctrines.

Thirdly, it has been demonstrated equally clearly, and far more cogently, that the sort of knowledge we call scientific, knowledge which has in it the characteristics of immanence and permanence, is founded on observation and experiment. The rise and growth of every science illustrate this fact. Even pure mathematics, commonly held to be the à priori science par excellence, and sometimes called 'the science of necessary conclusions,' is no exception to the rule. Those who would found mathematics on a higher plane have apparently forgotten to consider the contents of the mathematician's waste-basket. The slow and painful steps by which astronomy has grown out of astrology and chemistry out of alchemy; and the faltering, tedious, and generally hotly contested, advances of geology and biology have been made secure only by the remorseless disregard which observational and experimental evidence has shown for the foregone conclusions of the dogmatists and the literary opinions of the humanists. Thus it has been proved by the rough logic of facts and events that the rude processes of 'trial and error,' processes which many philosophers and some men of science still affect to despise, are the most effective means yet devised by man for the discovery of truth and for the eradication of error.

These facts are so well known to most of you, so much a matter of ingrained experience, that the categorical mention of them here may

seem like a rehearsal of truisms. But it is one of the paradoxes of human development that errors which have been completely dislodged from the minds of the few may still linger persistently in the minds of the many, and that the misleading hypotheses and the dead theories of one age may be resuscitated again and again in succeeding ages. Thus, to cite one of the simplest examples, it doubtless appeared clear to the Alexandrian school of scientists that the flat, four-cornered earth of contemporary myths would speedily give way to the revelations of geometry and astronomy. How inadequate such revelations proved to be at that time is one of the most startling disclosures in all history. The 'Divine School of Alexandria' passed into oblivion. The myth of a flat and four-cornered earth was crystallized into a dogma strong enough to bear the burden of men's souls by Cosmas Indicopleustes in the sixth century; it was supported with still more invincible arguments by Martin Luther in the sixteenth century; and it was revived and maintained with not less truly admirable logic, as such, by John Hampden and John Jasper in the last decades of the nineteenth century. To cite examples from contemporary history showing how difficult it is for the human mind to get above its primitive conceptions, one needs only to refer to the daily press. During the past two months, in fact, the newspapers have related how multitudes of men, women and children, many of them suffering from loathsome if not contagious diseases, have visited a veritable middle age shrine in the city of New York, strong in the hoary superstition that kissing an alleged relic of St. Anne would remove their afflictions. During the same interval a railway circular has been distributed explaining how tourists may witness the Moki snake-dance, that weird ceremony by which the Pueblo Indian seeks to secure rain in his desert; and a similar public, and officially approved, ceremony has been observed in the heat-stricken State of Missouri.

Such epochs and episodes of regression as these must be taken into account in making up an estimate of scientific progress. They show us that the slow movement upward in the evolution of man which gives an algebraic sum of a few steps forward per century is not inconsistent with many steps backward. Or, to state the case in another way, the rate of scientific advance is to be measured not so much by the positions gained and held by individuals, as by the positions attained and realized by the masses, of our race. The average position of civilized man now is probably below the mean of the positions attained by the naturalist Huxley and the statesman Gladstone, or below the mean of the positions attained by the physicist von Helmholtz and His Holiness the Pope. When measured in this manner, the rate of progress in the past twenty centuries is not altogether flattering or encouraging to us,

especially in view of the possibility that some of the more recently developed sciences may suffer relapses similar to those which so long eclipsed geography and astronomy.

It must be confessed, therefore, when we look backward over the events of the past two thousand years, and when we consider the scientific contents of the mind of the average denizen of this planet, that it is not wholly rational to entertain millennial anticipations of progress in the immediate future. The fact that some of the prime discoveries of science have so recently appeared to many earnest thinkers to threaten the very foundations of society is one which should not be overlooked in these confident times of prosperity. And the equally important fact that entire innocence with respect to the elements of science and dense ignorance with respect to its methods, have not been hitherto incompatible with justly esteemed eminence in the divine, the statesman, the jurist and the man of letters, is one which should be reckoned with in making up any forecast. It may be seriously doubted, indeed, whether the progress of the individual is not essentially limited by the progress of the race.

But this obverse and darker side of the picture which confronts us from the past has its reverse and brighter side; and I am constrained to believe that the present status of science and the general enlightenment of humanity justify ardent hopefulness if not sanguine optimism with respect to the future of scientific achievement. The reasons for this hopefulness are numerous; some of them arising out of the commercial and political conditions of the world, and others arising out of the conditions of science itself.

Perhaps the most important of all these reasons is found in the general enlargement of ideas which has come, and is coming, with the extension of trade and commerce to the uttermost parts of the earth. We are no longer citizens of this or that country, simply. Whether we wish it or not we are citizens of the world, with increased opportunities and with increased duties. We may not approve—few men of science would approve, I think—that sort of 'expansion' which works 'benevolent assimilation' of inferior races by means of a bible in one hand and a gun in the other; but nothing can help so much, it seems to me, to remove the stumbling blocks in the way of the progress of science as actual contact with the manners, the customs, the relations and the resulting questions for thought, now thrust upon all civilized nations by the events of the day. That sort of competition which is the life of trade, that sort of rivalry which is the stimulus to national effort, and that sort of cooperation which is essential for mutual protection, all make for the cosmopolitan dissemination of scientific truth and for the appreciation of scientific investigation. I would not dis-

parage the elevated aspirations and the noble efforts of the evangelists and the humanists who seek to raise the lower to the plane of the higher elements of our race; but it is now plain as a matter of fact, however repulsive it may seem to some of our inherited opinions, that the railway, the steamship, the telegraph and the daily press will do more to illumine the dark places of the earth than all the apostles of creeds and all the messengers of the gospel of 'sweetness and light.'

A question of profound significance growing out of the extension of commercial relations in our time is what may be called the question of international health. An outbreak of cholera in Hamburg, the prevalence of yellow fever in Havana or an epidemic of bubonic plague in India is no longer a matter of local import, as nations with which we are well acquainted have learned recently in an expensive manner. The management of this great international question calls for the application of the most advanced scientific knowledge and for the most intricate scientific investigation. Large sums of money must be devoted to this work, and many heroic lives will be lost, doubtless, in its execution; but it is now evident, as a mere matter of international political economy, that the cost of sound sanitation will be trifling in comparison with the cost of no sanitation; while further careful study of the natural history of diseases promises practical immunity from many of them at no distant day. International associations of all kinds must aid greatly also in the promotion of progress. Many such organizations have, indeed, already undertaken scientific projects with the highest success. Comparison and criticism of methods and results not only lead rapidly and effectively to improvements and advances, but they lead also to a whole-hearted recognition of good work which puts the fraternalism of men of science on a plane far above the level of the amenities of merely diplomatic life.

When we turn to the general status of science itself, there is seen to be equal justification for hopefulness founded on an abundance of favorable conditions. The methods of science may be said to have gained a footing of respectability in almost every department of thought, where, a half-century ago, or even twenty years ago, their entry was either barred out or stoutly opposed. The 'Conflict between Religion and Science'—more precisely called the conflict between theology and science—which disturbed so many eminent though timid minds, including not a few men of science a quarter of a century ago, has now been transferred almost wholly to the field of the theological contestants; and science may safely leave them to determine the issue, since it is evidently coming by means of scientific methods. The grave fears entertained a few decades ago by distinguished theologians and publicists as to the stability of the social fabric under the stress put upon it by the rising tide of scientific ideas, have not been realized.

And, on the other hand, the grave doubts entertained by the distinguished men of science a few decades ago as to the permeability and ready response of modern society to that influx of new ideas, have likewise not been realized. It is true that we still sometimes read of theological tests being applied to teachers of biology, and hear, occasionally, of an earnest search for a good methodist or a good presbyterian mathematician; but such cases may be left for settlement out of court by means of the arbitration of our sense of humor. It seems not unlikely, also, that there may persist, for a long time to come, a more or less guerrilla 'warfare of science' with our friends the dogmatists and humanists. Some consider this conflict to be, in the nature of things, irrepressible. But I think we may hope, if we may not confidently expect, that the collisions of the future will occur more manifestly than they have in the past in accordance with the law of the conservation of energy; so that the heat evolved may reappear as potential energy in the warmth of a kindly reasonableness on both sides, rather than suffer degradation to the level of cosmic frigidity.

Great questions, also, of education, of economic, industrial and social conditions, and of legal and political relations are now demanding all the light which science can bring to bear upon them. Though tardily perceived, it is now admitted, generally, that science must not only participate in the development of these questions, but that it alone can point the way to the solutions of many of them. But there is no halting ground here. Science must likewise enter and explore the domain of manners and morals; and these, though already largely modified unconsciously, must now be modified consciously to a still greater extent by the advance of science. Only within quite recent times have we come to realize an approximation to the real meaning of the trite saying that the proper study of man is man. So long as the most favored individuals of his race, in accordance with the hypothesis of the first centuries, looked upon him as a fallen, if not a doomed, resident of an abandoned reservation, there could be roused little enthusiasm with respect to his present condition; all thought was concentrated on his future prospects. How incomparably different does he appear to the anthropologist and the psychologist at the beginning of the twentieth century! In the light of evolution he is seen to be a part of, and not apart from, the rest of the universe. The transcendent interest of this later view of man lies in the fact that he can not only investigate the other parts of the universe, but that he can, by means of the same methods, investigate himself.

I would be the last to look upon science as furnishing a speedy or a complete panacea for the sins and sorrows of mankind; the destiny of our race is entangled in a cosmic process whose working is thus far only dimly outlined to us; but it is nevertheless clear that there are

available to us immense opportunities for the betterment of man's estate. For example, to mention only one of the lines along which improvement is plainly practicable, what is to hinder an indefinite mitigation, if not a definite extinction, of the ravages of such dread diseases as consumption and typhoid fever? Or what, we may ask, is to hinder the application to New York, Philadelphia and Chicago of as effective health regulations as those now applied to Havana? Nothing, apparently, except vested interests and general apathy. We read, not many years ago, that a city of about one million inhabitants had, during one year, more than six thousand cases of typhoid fever. The cost to the city of a single case may be estimated as not less, on the average, than one thousand dollars, making an aggregate cost to that city, for one year, of more than six millions of dollars. Such a waste of financial resources ought to appeal to vested interests and general apathy even though they cannot be moved by any higher motives. Thanks to the penetration of the enlightenment of our times, distinct advances have already been made in the line of effective domestic and public sanitation; but the good work accomplished is infinitesimal in comparison with that which can be, and ought to be, done. It is along this and along allied lines of social and industrial economy, that we should look, I think, for the alleviation of the miseries of mankind. No amount of contemplation of the beatitudes, human or divine, will prevent men from drinking contaminated water or milk; and no fear of future punishments, which may be in the meantime atoned for, will much deter men from wasting their substance in riotous living. The moral certainty of speedy and inexorable earthly annihilation is alone adequate to bring man into conformity with the cosmic rules and regulations of the drama of life.

And finally we must reckon amongst the most important of the conditions favorable to the progress of science, the unexampled activity in our times of the scientific spirit as manifested in the work of all kinds of organizations, from the semi-religious Chautauquan assemblies up to those technical societies whose programs are Greek to all the world beside. Literature, linguistics, history, economics, law and theology are now permeated by the scientific spirit if not animated by the scientific method. Curiously enough, also, the terminology, the figures of speech and the points of view of science are now quite common in realms of thought hitherto held somewhat scornfully above the plane of materialistic phenomena. Tyndall's Belfast address, which, twenty-seven years ago, was generally anathematized, is now quoted with approval by some of the successors of those who bitterly denounced him and all his kind. Thus the mere lapse of time is working great changes and smoothing out grave differences of opinion in favor of the progress

of science in all the neighboring provinces with which we have been able hitherto to maintain only rather strained diplomatic relations.

Still more immediately important to us are the evidences of progress manifested in recent years by this Association and by its affiliated societies. Our parent organization, though a half century old, is still young as regards the extent in time of the functions it has undertaken to perform. It has accomplished a great work; but in the vigor and enthusiasm of its youth a far greater work is easily attainable. Exactly how these functions are to be developed, no man can foresee. We may learn, however, in this, as in other lines of research, by methods with which we are well acquainted, namely, by the methods of carefully planned and patiently executed observation and experiment. The field for energetic and painstaking effort is wider and more attractive than ever before. Science is now truly cosmopolitan; it can be limited by no close corporations; and no domain of scientific investigation can be advantageously fenced off, either in time or in space, from the rest. While every active worker of this or of any affiliated society is, in a sense, a specialist, there are occasions when he should unite with his colleagues for the promotion of the interests of science as a whole. The results of the specialists need to be popularized and to be disseminated among the people at large. The advance of knowledge, to be effective with the masses of our race, must be sustained on its merits by a popular verdict. To bring the diverse scientific activities of the American Continent into harmony for common needs; to secure cooperation for common purposes; and to disseminate the results of scientific investigation among our fellow-men, are not less, but rather much more, than in the past, the privilege and the duty of The American Association for the Advancement of Science.

Viewed, then, in its broader aspects, the progress of science is involved in the general progress of our race; and those who are interested in promoting the former should be equally earnest in securing the latter. However much we may be absorbed in the details of our specialties, when we stop to think of science in its entirety, we are led, in the last analysis, back to the problem of problems—the meaning of the universe. All men 'gifted with the sad endowment of a contemplative mind' must recur again and again to this riddle of the centuries. We are, so to speak, whatever our prepossessions, all sailing in the same boat on an unknown sea for a destination at best not fully determined. Some there are who have, or think they have, the Pole Star always in sight. Others, though less confident of their bearings, are willing to assume nothing short of second place in the conduct of the ship. Others, still less confident of their bearings, are disposed to depend chiefly on their knowledge of the compass and on their skill in dead reckoning. We of the last class may not impugn the motives or doubt the sincerity

of the first two classes. We should find it difficult, probably, to dispense with their company in so long a journey after becoming so well acquainted with them; for among them we may each recall not a few of those rarer individuals of the genus homo called angels on earth. But it must be said in all truth, to resume the figure, that they have neither improved much the means of transportation nor perfected much the art of navigation. They have been sufficiently occupied, perhaps, in allaying the fears of the timid and in restraining the follies of the mutinous. Other types of mind and other modes of thought than theirs have been essential to work out the improvements which separate the earlier from the later nautical equipments of men; such improvements, for example, as mark the distinction between the dug-out of our lately acknowledged relatives, the Moros and the Tagalogs, and the Atlantic-liner of to-day.

At any rate, we are confronted by the fact that man's conceptions of the universe have undergone slow but certain enlargement. His early anthropocentric and anthropomorphic views have been replaced, in so far as he has attained measurable advancement, by views that will bear the tests of astronomy and anthropology. He has learned, slowly and painfully, after repeated failures and many steps backward, to distinguish, in some regions of thought, the real and the permanent from the fanciful and fleeting phenomena of which he forms a part. His pursuit of knowledge, in so far as it has led him to certainty, has been chiefly a discipline of disillusionment. He has arrived at the truth not so much by the genius of direct discovery as by the laborious process of the elimination of error. Hence he who has learned wisdom from experience must look out on the problem of the universe at the beginning of the twentieth century with far less confidence in his ability to speedily solve it and with far less exaggerated notions of his own importance in the grand aggregate of Nature, than man entertained at the beginning of our era. But no devotee to science finds humiliation in this departure from the primitive concepts of humanity. On the contrary, he has learned that this apparent humiliation is the real source of enlightenment and encouragement; for notwithstanding the relative minuteness of the speck of cosmic dust on which we reside, and notwithstanding the relative incompetency of the mind to discover our exact relations to the rest of the universe, it has yet been possible to measure that minuteness and to determine that incompetency. These, in brief, are the elements of positive knowledge at which we have arrived through the long course of unconscious, or only half-conscious, experience of mankind. All lines of investigation converge toward or diverge from these elements. It is along such lines that progress has been attained in the past, and it is along the same lines that we may expect progress to proceed in the future.

'FREE-WILL' AND THE CREDIT FOR GOOD ACTIONS.

By Professor GEORGE STUART FULLERTON,
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WE can imagine what would be the emotion of a college professor if the president of the institution were to shake him warmly by the hand and congratulate him upon the fact that, although he had been freely admitted for a year to the book-stack of the college library, he had not stolen so much as a single volume. We can conceive the embarrassment of a clergyman whose bishop would feel impelled to give expression to his satisfaction at the fact that, during a visit extending over a week or more, he had not been distressed by hearing any word of profanity or observing any act of violence. 'What in the world can the man have been expecting of me?' exclaims the indignant recipient of such a compliment. 'Does he take me for a blackleg? Perhaps the next time he sees me he will take it upon himself to felicitate me on having so far escaped the gallows.'

The fact is that whenever we speak of a man as deserving credit for this or that worthy action, our compliment is accompanied by something very like a criticism. It is implied that the action is one not easy to perform, at least in the given instance. We do not ordinarily think a mother deserves great credit for taking good care of her infant, or a father for supporting his family when he has it in his power to do so. We assume that these things are easy and natural, and quite in accord with the impulses which control the individual. But we do think a woman deserves credit for adopting and lavishing her care upon motherless children that have no special claim upon her, and a man for laboring to feed those who are not bound to him by the closest of natural ties. Were it as easy to care for the children of others as to care for one's own, were the impulse to do so just as strong, we should never think of such actions as especially creditable. They would undoubtedly be good actions, but it is one thing to recognize actions as good and quite another to single them out as deserving of credit. It is a good thing for a college professor not to steal, and for a clergyman to avoid acts of violence, but we never think of remarking upon the fact that their conduct is creditable, when we have nothing better to say of them than that they possess these negative virtues.

Some virtues we expect of men generally. We assume that the right course is the easy one to take, or, at least, is relatively easy, and we

scarcely think of praising the average man for coming up to this standard. Some virtues we expect of certain classes of men, and not in the same degree of others. We expect, for example, a greater foresight and power of self-control in those who have enjoyed educational advantages and whose horizon has been widened. And we expect a much greater sensitiveness to moral considerations on the part of those who have had the inestimable advantage of good moral training, and have been brought up in a good home. We judge a man according to the class in which we find him; if he falls below the expected standard of excellence, we blame him, and if he rises above it, we regard him as worthy of credit. But besides these class-standards, which may be very numerous, we have individual standards which we apply when we come to know individual men well, and these express our judgment of the actual moral condition of the individual.

We have, perhaps, known our friend Smith for ten years or more, and have clearly perceived that he readily falls a prey to irascible impulses. He himself deplores the fact, and resolves to express himself more temperately when things happen to ruffle him. We see him on some trying occasion with flushed cheek and the flash in his eye that has heretofore heralded the tempest. But the expected storm does not come; the good resolution has triumphed, and the clouds roll away without emptying themselves as the weather-wise had fully expected them to do. Of course we give Smith no little credit for this victory, and if we really know him intimately we probably endeavor to let him know, in some tactful way, that we admire his magnanimity and self-control. Or perhaps the individual to whom we give credit for a rather unexpected act of self-denial is our son Tommy, whom we have known rather intimately for a number of years, and with whose impulses and capabilities we think we are fairly well acquainted. The boy has on various occasions found stolen jam irresistibly sweet, and neither reflections upon the possibilities of detection and punishment nor the feeble stings of an immature conscience have sufficed to deter him from tasting that sweetness. But we discover that, on a certain occasion, opportunity has not been lacking. There has been a prolonged conflict between the law in his members and the law in his mind, and the latter has come off victorious. We praise Tommy for his continence, make him feel that he has left the field covered with glory, and we devise means of implanting in his small mind the conviction that honesty is not a thing to be regretted.

In this last instance we have, I think, a good indication of what we really mean by the credit that is given to this or that good action, and of the standard by which we measure it. We think of an action as creditable when we recognize the presence of warring impulses, and regard the good decision as a victory over a more or less redoubtable enemy. The

more evenly balanced the force in the field, the more creditable we consider a choice of the right. When we feel personally responsible for the conduct of the individual concerned, we recognize the degree of credit he has earned as a moral claim upon us for payment in coin of some sort. The payment may consist in expressions of approval, in evidences of confidence or of affection, in marks of respect; or it may consist in a large portion of jam at the next distribution, a visit to the circus or a trip to the country. This payment, which parents and teachers do not fail to make if they properly realize their responsibilities, is not made because the child *is good,* for good actions performed easily and without a struggle are not singled out for reward in this way. It is made because the child *needs to be made good,* and we roughly proportion the reward to the amount of encouragement needed to keep the child moving along the path of moral development.

In the larger world beyond the nursery and the school, rewards for creditable behavior are not always distributed in the same unmistakable way. A good deal of creditable behavior appears to be unrewarded. The reason is not far to seek. Men generally are not occupied in educating each other just as parents and teachers educate those under their charge. They have not the same sense of responsibility; and, further, they have not, in many cases, the power to grant rewards. But it is easy to see that, where men are at all sensitive, as civilized human beings surely ought to be, to the moral or immoral character of the actions of their fellows, they are quick to judge of actions as creditable or discreditable, and they have the disposition to mete out to the doer some sort of reward or some sort of penalty. The reward may be no more than a look of admiration or a word of appreciation, and the penalty no more than a slight coldness of manner; but love of approbation is a strong motive to action, and just such rewards and penalties as these may have an enormous influence in determining to right conduct. And where certain men exercise over others a control at all analogous to that exercised by the parent or teacher, we find that they are very apt to reward creditable behavior much as these do. The unusual devotion of this or that employee, the conspicuous bravery of the soldier, are not commonly passed over as matters that deserve no substantial recognition. The good behavior of the convict is accounted as sufficient reason for shortening the term of his imprisonment. Look where we will, we find that there is a general tendency among men to regard the creditable actions of their fellows as having some sort of a claim to reward, and when we look into the nature of this claim, we find that its force rests upon the fact that we instinctively regard ourselves as in some way responsible for the behavior of others, and, consciously or unconsciously, take it upon ourselves to encourage them to act as they should act.

Now we not infrequently hear that, if the position taken by the determinist is right, our notions of the creditable or discreditable character of actions must be wholly erroneous. What the determinist really holds I have tried to make clear in an earlier number of this magazine.* He holds that human actions could be completely accounted for if we really knew all their antecedents. Among these antecedents he reckons the character, the inherent or acquired impulses, of the individual. It is only the fatalist that overlooks these, and fatalism is something very different indeed from determinism. The determinst maintains that the question: 'Why did this man act in this particular manner?' is never a foolish question, although we may in any particular instance be ignorant of the answer. He assumes that there is always some cause or causes that can account for the result. The 'free-willist,' on the other hand, maintains that no complete answer to such a question can be given, not because we are ignorant, but because human actions are not necessarily the results of causes. If we ask him: 'Why did this man elect to put his hand in his pocket and take out a copper for the beggar on the street?' he is capable of answering: 'Just because he did,' and this 'because' is no better than a 'woman's reason,' i. e., it is no reason at all. It amounts to asserting that, in so far as human actions are 'free,' they have no cause whatever, and the search for an explanation of their occurrence is wholly futile.

But what can induce any man to hold that we cannot regard actions as creditable in so far as they can be accounted for by antecedents of some sort, and that we must regard them as creditable only in so far as they are causeless? The position is one often enough taken, and probably there is no one of my readers who has done some reading in ethics who has not met with this opinion. It is clear that there is nothing in what I have said above about the credit we allow to good actions, that cannot be assented to by a determinist. He admits that men differ greatly in character, and that, in the same circumstances, two different men may act in very different ways. He admits that men's characters may change, and thinks it his duty to influence them to change in the proper direction. Rewards and punishments he regards as a part of the machinery which brings about the gradual moralization of the race. He sees no objections to distributing rewards where they will do the most good and the least harm; and he points to the actual practise of mankind in evidence of the fact that men generally have unconsciously embraced the principle upon which he insists, and do constantly act upon it. Yet the 'free-willist' maintains that he is wholly in error, and that credit and discredit must be allowed upon a very different principle. Does the 'free-willist' take this position 'freely,' i. e., for no reason at

* December, 1900.

all? or may we assume that he does so because it seems to him at least a plausible one? If he says what he does just 'because he does,' it is, of course, useless to argue with him. He is not what we call a rational being, and is not moved to embrace this or that conviction by evidence. But, being myself a determinist, I will be more generous with him than he is with himself, and will maintain that he is not so wholly unreasonable as he represents himself to be. I will look for some motive which may explain why he takes so strange a position.

A very little reflection upon what 'free-willists' have written reveals that that motive is not far to seek. It is the old confusion of indeterminism, or 'freedom' in a special sense of the word, with freedom in the usual sense, freedom from compulsion. No man in his senses thinks of praising or blaming any one for acts performed under compulsion. If a stronger than Tommy seizes his small hand, forces his fingers to close upon a key and turn it, pries open his mouth and fills it with jam, no sane parent would dream of punishing the involuntary offender. And if a stronger hand catches the boy as his fingers are stealing towards the lock, and drags him forcibly away from the fascinating spot, no one but a fool would regard the precipitate retreat as a triumph of virtue that calls for the crown of some substantial reward. It may or may not be a desirable thing to be born with red hair, but surely no one will maintain that it is a creditable thing. When he is acting under compulsion, Tommy's actions are no more a matter of choice than is the color of his hair, and we recognize this fact in judging him. On this point all classes of moralists are agreed—actions can be creditable or discreditable only if they are voluntary, or only if the actor is free.

We ought never to forget, however, that freedom in this sense of the word means only freedom from compulsion, a freedom to act out the impulses inherent in one's own nature. It is a totally different thing from 'freedom,' that philosophical fiction that has played so large a part in polemical literature. But it is easy to confuse things that pass by the same name, and when the 'free-willist' hurls at us the contemptuous question: 'Do you mean to assert that there can be any credit for actions which we do not freely do?' we too often make haste to affirm that there cannot be, without stopping to ask him whether he means the word *freely* to be understood with or without the quotation marks. He himself fails to perceive that the word is ambiguous; and seeing, as we all do, that only free actions are deserving of credit, he makes this true of 'free' actions. He thus comes to deny credit to every action that is not causeless. It is evident that he has no *good* reason for such an assertion, but he has at least *a* reason; he has simply fallen into a confusion, and to do this is human, while to embrace a doctrine 'freely,' or for no reason at all, appears positively inhuman.

We have seen that, from the point of view of the determinist, it seems an eminently reasonable thing to regard certain good actions as deserving of credit rather than others, and to strive to reward them. We have seen also that we can estimate roughly, at least, the amount of the reward that it is desirable to give. There appears to be nothing absurd, and nothing hopelessly mysterious in the whole matter, although our ignorance of human character, its impulses, the motives that can be expected to lead to this or that action, and, indeed, of the whole machinery of human life, is and must remain very great. But what if we adopt the hypothesis of the 'free-willist'? Let us suppose for the moment that actions can be regarded as creditable only in so far as they are 'free' or causeless, and let us see whether this will cast a brighter light upon the corner of ethics with which we are concerned.

The first difficulty which meets us is a seemingly hopeless uncertainty as to what actions are 'free' and the degree of their 'freedom.' We watch Tommy from a distance as he loiters about in the region of the pantry. Evidently there is a struggle going on within him. He advances his hand; he withdraws it; he takes a step forward; he looks about apprehensively; he touches the key; he stops to reflect. Finally he sighs, and walks away without having done the deed. Of what warring forces has his little mind been the theater? Were the combatants but two—love of jam and 'free-will'? Can we measure the amount of the latter by the degree of opposition which it has met and overcome in the former? Certainly not. Tommy has been whipped before for this offense. He has been talked to seriously on many occasions, and he is not a bad-hearted boy. Fear of detection may influence him more or less; the beginning of a love for virtue and a rather well-developed love of approbation count for something. He has within him a germ of self-respect. All these things are enlisted on the side of right conduct, and the potent influence of just such forces as these even a 'free-will' parent frankly recognizes. No philosopher who has had the fortune to have a son, and who has cared anything about him, has ever delivered him over bodily to the tender mercies of 'free-will.' He keeps prodding at 'free-will,' so to speak, in a more or less deterministic way. It may be his trump card, but he is never willing to throw away the rest of his hand. Accordingly, we must assume that the battle has not been a duel, but a general mêlée. What measure of credit can 'free-will' assume for the result? How shall we apportion our reward for the victory? Does the boy deserve *no* credit except in so far as he has acted 'freely'?

Moreover, how are we even to know which action should be rewarded? The determinist has no great difficulty in picking it out, for the mere sight of the struggle is to him an indicatioon that encouragement is needed and should be given. The 'free-willist' can, of

course, not regard the reward as an encouragement, for it is foolish to attempt to encourage 'free-will.' Could it be 'encouraged' it would evidently not be 'free.' And if this notion of encouragement wholly drops out, there appears no reason why actions performed after a struggle should be regarded as creditable rather than actions performed without any struggle at all. Suppose that Tommy has attained such a fixity of character that he can pass the pantry door twenty times without apparent effort. Does this mean that he has lost his love of jam? May it not mean that he is subject to such generous bursts of 'free-will' that all fleshly inclinations are overcome as soon as they are born? Then why should he not be rewarded more generously than before, when he had such dribblings of 'free-will' as scarce sufficed to bring him out of the combat alive?

It appears, then, that it is impossible to ascertain how much credit is to be allowed for any action, and that it is impossible to discover what actions are to be regarded as creditable. This does not seem encouraging, and may well tend to dampen our 'free-will' ardor. But we must pluck up our courage, for we are compelled to face a difficulty which is, if possible, more disheartening, Reflection discloses the fact that our theory forces us to deny the validity of the moral judgments that we have all our lives been passing upon our own actions and those of our fellows. This is so important a point that I must try to make it quite clear. It is a point passed over in silence by the 'free-willist.'

Let us suppose that Smith sees Jones struggling in the water, and makes desperate efforts to save him from drowning. His efforts are crowned with success, and Jones sits dripping on the bank, with a heart overflowing with gratitude. But he speedily discovers in Smith a creditor whose sole interest in the transaction was a pecuniary one. He saw his money drowning before his eyes, and he did his best to secure it. Does Jones now owe the man both money and gratitude, or does he owe him money alone? Let us suppose again that we have contemplated with satisfaction the temperate and orderly conduct of a young man whom we have regarded as exposed to divers temptations. We feared he was going to be dissipated, and we have been agreeably disappointed. We give expression to our pleasure, and he informs us frankly that the least rumor of misconduct would lead his uncle to disinherit him. 'Wait,' he says 'until the old man dies, and you will see my good time begin.' Do we, after this avowal, regard him as a model of virtue, and a youth to be held up as a pattern? No man rates as a philanthropist the scientific enthusiast who visits the sick with assiduity only in order to secure materials for his contemplated monograph on pain. Before we judge of human actions we try to find out something about their setting. We pry into motives and inquire regarding intentions. Precisely the same act may be good or bad, according to its context. It

is not a moral act for a savage to save a man alive if he is spared with the intention of fattening and eating him later.

Now let us suppose that the action under discussion is my contribution of a dollar to the hoard of the beggar on the corner. Is it a creditable action? Is it even a moral action? Only the unreflective will undertake to answer off-hand that it is. I may have given that dollar in the hope that one more drinking-bout would finish the beggar, and relieve me of his unæsthetic presence when I take my daily walk. I may have given it out of pure vanity, and to compel the admiration of the pleasing young person who is waiting for the tram. On the other hand, I may have given it because I was touched by the sight of suffering, and was willing to make a sacrifice for the sake of relieving it. It seems the most natural thing in the world to judge that the action was, in the last case, a creditable one, but was not creditable in the others. We have been judging of actions in this way all our lives.

But what if the act was a 'free' one? What if it was not determined by my character and impulses and the peculiar circumstances in which I was placed? In this case it cannot be explained by my desire to be rid of the beggar's presence. The impression made upon me by the fair onlooker cannot account for it. The sight of the beggar's misery furnishes no explanation. We cannot ask *why* the act was done. It was a *'free'* act. It simply appeared. It was not done for the sake of removing the beggar, tickling my vanity or relieving suffering; for just in so far as an act is 'free' it cannot be accounted for by any ideas antecedently in my mind or by my natural tendency to selfishness, to vanity or to generous movements of sympathy. It is, hence, an act without a setting—causeless, purposeless, blind. Is it a creditable act? Are such acts the *only* creditable acts? Surely we have turned our face resolutely away from the moral judgments of mankind when we have committed ourselves to the unnatural doctrine that only 'free' acts are deserving of credit.

It is quite inconceivable that men should with open eyes defend the doctrine of 'freedom' on moral grounds. When they attempt to do so, it is clear that they are really arguing in favor of *freedom*, a thing well worth fighting for, and dear to the heart of determinist and 'free-willist' alike. They have simply fallen into a confusion, and have confounded two things that are extremely unlike. I should be the last to maintain that the world could get on properly without philosophers, but I must frankly admit that the philosopher sometimes falls into error, and is very apt to take with him in his fall certain of the by-standers who, if left to themselves, would never have thought of tumbling into that particular ditch.

Helmholtzian Fog Billow, named in honor of von Helmholtz, who first discussed such Conditions.

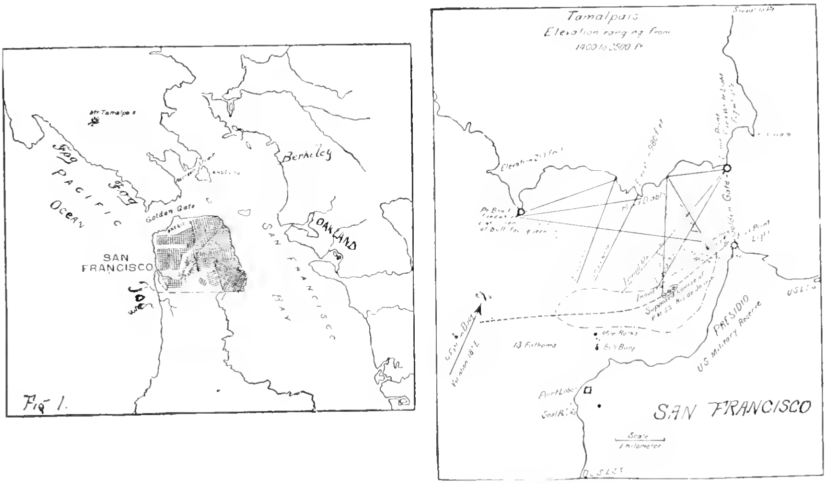
FOG STUDIES ON MOUNT TAMALPAIS.

BY ALEXANDER McADIE.

LATE on a February afternoon the passengers on a large Pacific Mail steamship sighted the Farallones and doubtless thought as the pilot came aboard that the long run across the broad ocean not always true to its name was safely over and danger past. The 'Rio de Janeiro' came to anchor a little before six o'clock on Thursday night, February 21, 1901, and the weather being foggy, the captain wisely remained at anchor until about 4 a. m. when the fog lifted. The lights of the Cliff House two or three miles away could be seen and the vessel started on a northeast course with Lime Point dead ahead. There is some difference of testimony as to whether the Captain or Pilot gave the order to go ahead. The fog closed down again and the Pilot steered by the whistle hoping to get the echo from Point Diablo. No echo was heard. The vessel was not moving at full speed, the First Officer was standing on the starboard side listening for the Fort Point bell and the Captain and Pilot were on the bridge. No soundings however were taken. At about 5:30 a. m. the vessel struck the Fort Point Reef, backed off and within twenty minutes had gone from sight with 130 of the 210 persons aboard.

The two diagrams herewith show the general approach to the Bay of San Francisco and in more detail the probable path steered by the 'Rio' with zones of inaudibility of the fog signals.

When all is said and done it appears that the fog was the prime cause of this appalling accident. Now, while an accident of such magnitude gives startling emphasis to the need of studying fog, a summation of the minor accidents for a single year due to fog in any large seaport would be equally impressive. One cannot cross the sea, run down the coast or even go over a bay upon a ferry-boat without experiencing at times this troublesome condition. Nor is it only when on the water that we are at the mercy of the fog. Study the statistics of railway accidents and you will be surprised how often, in the column giving the cause of collision or other accident, the word fog appears. Can we help ourselves? Yes; and the first step is to study patiently and systematically the various types of fog formation. Already the ability to communicate by means of wireless telegraphy between vessels at sea and the land removes the greatest element of danger to vessels caught in fog. The 'Rio de Janeiro' was lost at the entrance to

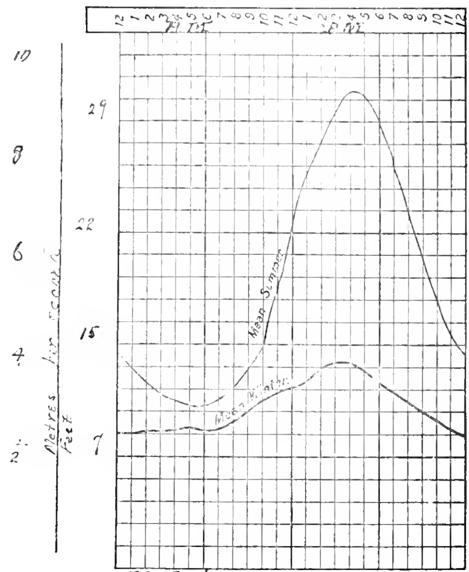


THE REGION ABOUT SAN FRANCISCO AND PROBABLE CONDITIONS AT TIME OF WRECK.

PROBABLE COURSE OF 'RIO DE JANEIRO.'

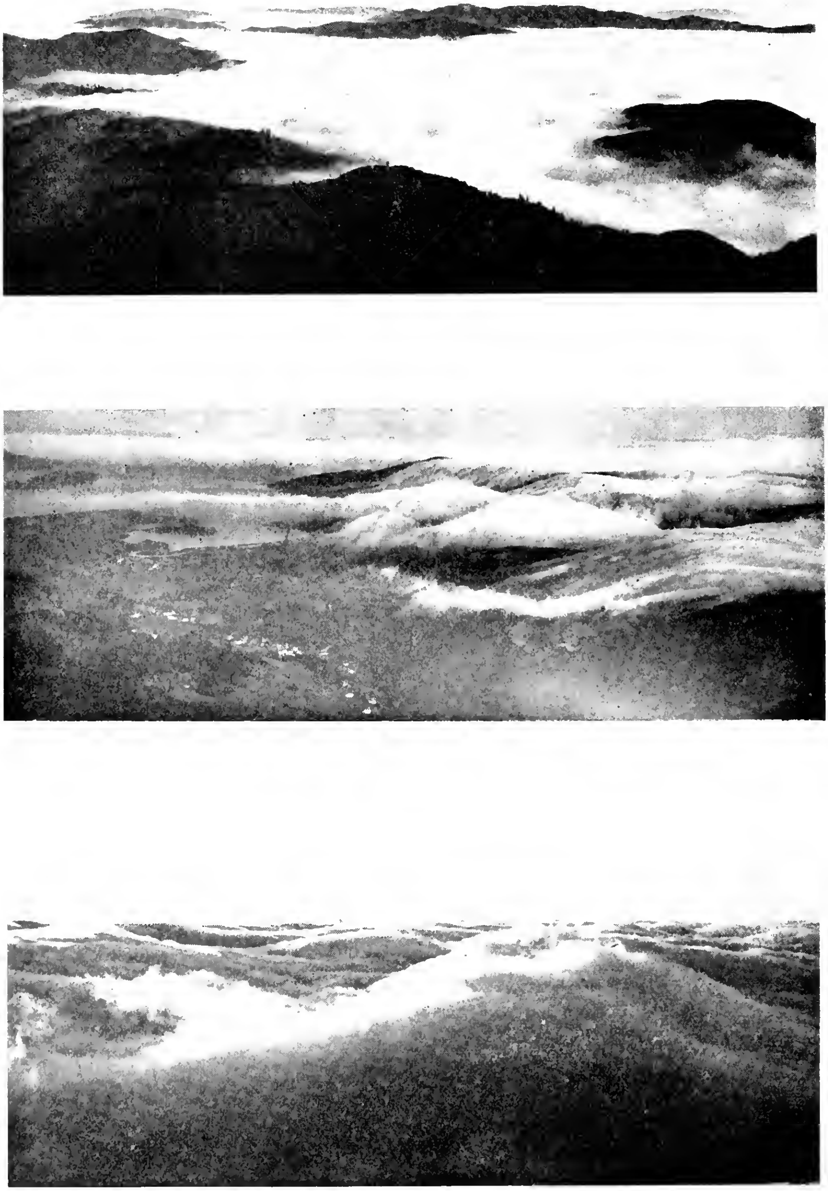
what might have been thought to be a well-protected harbor. That is, there were light-houses and fog whistles along the shore, but the vessel was helpless, nevertheless, when the fog closed down, for all guiding points were lost and, owing to the peculiar reflections and refractions of sound waves in the air, the whistles and bells, as the accident too sadly proved, were inaudible.

In the vicinity of San Francisco the processes of cloudy condensation in the free air are very active. It is no uncommon occurrence on summer afternoons, when the wind is blowing at the rate of twenty-two miles an hour, to see sharply marked fog drifts hang like white blankets over the city hills or stream through the Golden Gate like a spectral army. From the U. S. Weather Bureau Observatory on Mount Tamalpais, elevation about 2,400 feet, one looks down upon such remarkable fog formations as are shown in the accompanying illustrations.



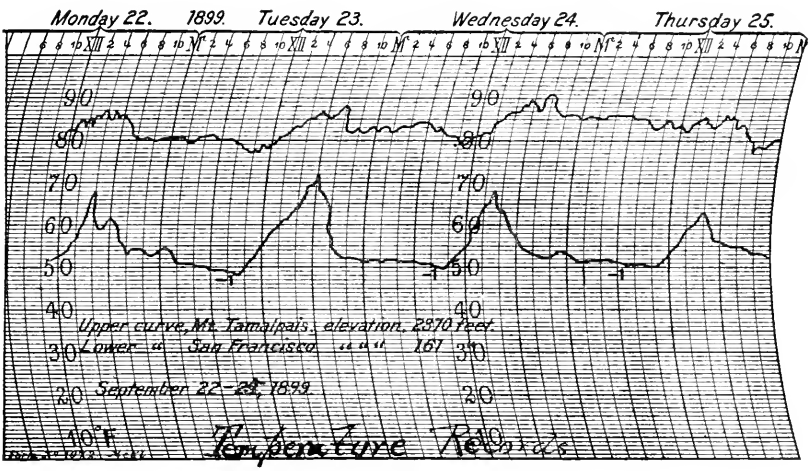
Hourly Wind Velocity at San Francisco

Now fog, like frost, may be considered to be largely a problem in *air drainage*. The condensed vapor, like the frozen vapor, indicates air motion with certain accompanying changes in temperature. Therefore the first line of study in connection with fog formation is concerned with temperature gradients; and chiefly the vertical gradient. Instead of the usual fall in temperature of 1° for each 183 feet elevation, we find in these San Francisco fogs an increase of temperature from sea-level upwards. In a given summer month the mean daily temperature at the upper station was eleven degrees or more warmer than at the lower station. If the rate of increase were uniform throughout the 2,500 feet, this would mean a rise of one degree for two hundred feet elevation. The rate is not uniform, and between the 1,500 feet and 2,000 feet levels is probably often as much as one degree for fifty feet. Days without fog are as a rule days without this steep inverted gradient, and it would seem as if the temperature throughout the entire mass of air was more uniform. Some approximate vertical sections of the temperature in a fog bank were obtained by carrying a Mar-



FOG STUDIES FROM MOUNT TAMALPAIS.

vin meteorograph from the summit to the valley and back, the descent and ascent requiring about 100 minutes, the distance being about 16 miles. The instrument was hung at the top of an open canopied car in such a way as to secure a good air circulation. The average dew-point was 50° F. (10° C.) and the maximum weight of a cubic foot of water vapor at this temperature and saturation 100 per cent. is a little over 4 grains. Estimating an average fog bank as covering an area of fifty square miles and extending from the 500-foot level to the 1,500-foot level, the maximum weight of the water vapor would be about 400,000 tons; and if this condensed vapor could be suddenly precipitated it would be the equivalent of a rainfall of about one-tenth of an inch. But condensation and precipitation are not identical. The processes



TEMPERATURE RECORDS.

which cause the collapse, if it may be so called, of a cloud of fog are obscure. Elaborate experimentation is needed at this point before the problems of fog dissipation or rain-making can be solved. With the fogs of the San Francisco Bay district and indeed with every dense cumulo-nimbus or cumulus cloud, the condensation is considerable and it would seem at times as if but a very gentle initiative would lead to precipitation. Various methods of removing dust particles from the atmosphere have been suggested within the past few years, and possibly in thus removing the dust the essential nuclei of condensation may be removed. Conversely, in order to bring about precipitation, it may be found necessary to supply at the proper time the proper nuclei. At any rate, it is known that by various methods, of which may be mentioned filtering, clarifying, recondensing, calcining and electrifying, smoke, fog, dust and condensed vapor may be removed from limited spaces. The

removal of the fog for even a small distance in the neighborhood of a fog-bound vessel might be of advantage. The chief difficulty at present would seem to be the quick influx of the circumjacent fog. The supply of fog might be so great that our dissipators would seemingly produce no effect. The dissipation of fog and smoke in enclosed areas by electrical agencies, as strikingly shown in Dr. Lodge's experiments, leads to the wish to reproduce these experiments in the free air and upon a large scale. Moreover within the past two years there has been growing up a theory due chiefly to Zeleny, Elster, Geitel and T. C. R. Wilson concerning the part played by ions in causing rain. It is known that the negative ions move more rapidly than the positive ions and that water vapor will condense more readily on the negative ions. It may

SUNSET OVER A SEA OF FOG.

be that under certain unstable conditions some of the more energetic ions, by relieving the electric tension, inaugurate the formation of the rain-drop. In studying the electrical potential of the atmosphere, it has been shown that the approach or retrocession of clouds, especially cumulus and cumulo-nimbus, could be determined by the changes in the potential values. There was also good reason for believing that the electrometer gave in certain fluctuations indications of the proximity of invisible vapor masses. Certainly the one instrument upon which we now rely in studies of fog formation and influence, the mercurial thermometer, is far from being a sufficiently sensitive instrument. Optical methods may furnish apparatus sufficiently delicate. It has

been claimed by some that the polarization of blue sky light can be used in studying the vertical distribution of fog, and that changes in atmospheric conditions are shown by this means several hours in advance of other precursory appearances.

Strangely enough within the past year and from an unexpected source, suggestions have been made which should be considered with some care and then tested. In discussing the mortar batteries used at Windisch Feistritz, Dr. Pernter has given us some data concerning vortex rings. These are the rings which, according to Burgermeister Stiger and his associates, successfully protect their vineyards from hail. Whatever the real cause may be regarding hail, we are thankful for the opportunity to study such large and energetic vortices. These rings are powerful enough to tear a thick paper screen to pieces at a distance of 100 meters. On leaving the mortars in a horizontal direction the whirls have a velocity of about 170 miles per hour or eight times the velocity of the stiff surface indraft of air on summer afternoons through the Golden Gate. At a distance of 100 meters the velocity was reduced nearly 50 per cent. With the Suschnig apparatus, the charge of powder being 250 grammes, Dr. Pernter found an initial velocity of about 55 meters per second. The probable limit of upward movement was 400 meters. Dr. Hann has suggested that the results obtained by shooting these rings into winter fogs should be carefully studied. The suggestion is pertinent. At Mt. Tamalpais, as we have tried to show, unusually good opportunities exist for experimenting upon fog. Many varieties of formation occur. The tule fogs of winter, in one of which the 'Rio de Janeiro' was lost, sometimes do not exceed 100 feet in depth. The summer afternoon sea fogs are more dense and more sharply defined. Some of the fogs are due to direct cooling by contact; in some the cooling is due to radiation, and, in the great majority of cases, the cooling is due to mixture. The differences in temperature, humidity and air motion are so marked that it is likely that differences in electrical potential, dust-content and ionization also exist. There is urgent need of bettering our knowledge of these matters. Practical applications will speedily follow.

The sacrifice of life on that ill-fated steamship on the morning of February 22 will not have been altogether in vain, if it leads to a thorough study of the conditions governing fog.

THE FRENCH SARDINE INDUSTRY.

By Dr. HUGH M. SMITH,

U. S. COMMISSION OF FISH AND FISHERIES.

AMONG the foreign fishery industries on which Americans are dependent for a part of their food supply, few exceed in interest or importance the sardine industry of France. The value of the French sardines imported into the United States is about one million dollars annually, and the wholesome, palatable and convenient canned sardine is consumed in nearly every community. The accompanying notes on the sardine and the industries to which it gives rise are extracted from an article in the 'Bulletin' of the United States Fish Commission for 1901, based on the writer's personal observations in Brittany, the principal center of the sardine fishery.

The sardine is the leading fishery product taken in the waters of France. From official statistics it appears that in 1898 the sardine fishery gave employment to 31,871 fishermen; the number of boats used was 8,164, valued at 5,934,633 francs; the apparatus employed was worth 7,030,945 francs; the quantity of sardines taken was 53,924,-275 kilograms (or 118,633,400 pounds); and the selling price of the fresh fish was 9,204,988 francs (or about $1,840,997).

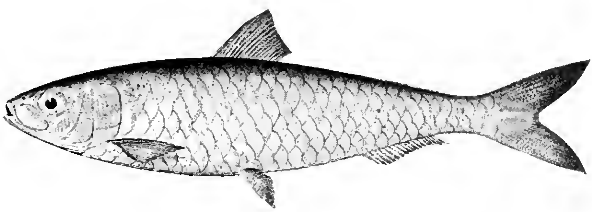
There exists considerable uncertainty among the fishing interests and the general public in America and Europe regarding the sardine of the Bay of Biscay and the Mediterranean Sea. Some persons have believed that the sardine canned in France is a distinct species, while others have held that the French sardine, like the sardine of New England, is simply the young of some herring-like fish. The term sardine is a general one, applied to various clupeoid fishes, mostly of small size, in different parts of the world, and can not be restricted to any particular fish. Thus, there are the Spanish sardine of the West Indies and Florida; the California sardine, found along the entire west coast of the United States; the Chile sardine; the oil sardine of India; and the sardines of Japan and New Zealand. But the sardine *par excellence* is the French sardine, called also celeren, celan, royan, galice and cradeau on various parts of the French coast. The name sardine has reference to the island of Sardinia, in the Mediterranean, about whose shores the fish is abundant.

As early as 1553, Pierre Belon, a French naturalist, asserted that the sardine is the young of the pilchard; and this is the view now held

by nearly all authorities. The pilchard, as is well known, is one of the most important fishes of the southern coast of England, being especially abundant in Cornwall. Young pilchards or 'sardines' are found on the Cornish coast, but are apparently not so numerous as in France and are in little demand, as canning is very limited in extent; on the other hand, large sardines or pilchards are caught on the French coast, but are much less abundant and less important than the small fish.

In allusion to the small sardine being caught almost wholly by means of bait consisting of fish roe (rogue), the French call it *sardine de rogue,* in contradistinction to the large fish which is taken without bait by means of drift nets, and hence called *sardine de derive.* Modern French writers on the sardine fishery seem averse to acknowledging the specific identity of the sardine and the pilchard; some even fail to explain or suggest the relation between the large and small fishes of the west coast of France.

The pilchard is a well-marked species, easily distinguished by prominent radiating lines on the operculum and by large scales, as well as by other features. The usual length is eight or nine inches; the length of the largest recorded specimen was fourteen inches (taken in Cornwall). The sardine of the French coast is a handsome little fish, whose beauty is not entirely lost in canning. In the water the back is of a greenish color, but out of the water the upper parts are rich dark bluish, contrasting strongly with the silver and white of the sides and abdomen. The scales are very easily detached, but their loss does not detract seriously from the appearance of the fish, when either fresh or canned, as the skin is rather thick and has a brilliant uniform silvery color.



PILCHARD OR SARDINE (*Clupea pilchardus.*)

The range of the sardine extends from Sweden to the Madeira Islands. The southern coast of England, the Atlantic coast of France, and the Mediterranean Sea are the chief centers of abundance.

On the coast of Brittany the *sardine de rogue* is found about nine months of the year, being absent from the inshore waters most of the winter. When the fishing season opens, the fish are reported first at Arcachon and other southern points on the west coast, and gradually reach the districts toward the north. During the winter, however, the

large fish—some a foot in length—are observed at various places on the coast.

The immature sardines frequent the coast waters throughout the summer and remain in Brittany until late in fall. Some years, if the season is mild, they are caught until the first or second week in December, but a storm coming any time in November is likely to drive them away and terminate fishing for the season. In 1900 sardine fishing at Concarneau was ended November 5— the same date as in 1899 —by a southwest storm, which swept away all the sardines in the bay.

The spawning time on the coasts of England and France is from June to October. Spawning takes place at a considerable distance from the land, and ripe or spawning fish are seldom caught, as fishing is done mostly in the inshore waters. The small fish used for canning purposes on the French coast are never found with ripe eggs or milt, and are now known to be immature fish hatched in the summer and fall of the previous year. The eggs are buoyant, and the average number extruded is reported as 60,000. In the Mediterranean the sardine apparently belongs to a different race, which is smaller than the oceanic form and reaches maturity when under 7 inches in length.

When sardines first arrive they are poor and unsuitable for canning; but as the season advances they improve in quality, and are fatter in September than in June and in December than in September. Their food consists mainly of copepods and other small crustacea. Small fish eggs are also a favorite food. The fondness of the sardine for such eggs plays an important part in the fishery.

The sardines go in schools and swim at or near the surface. As many as 100,000 fish have been taken in one net from one school, but the usual catch is much less. They are preyed upon by cetaceans and by many fish—the mackerel, the haddock and the dolphin being especially destructive on the French coast.

Like other free-swimming oceanic fish, the sardine varies in abundance from year to year; but there is no evidence that the fishing is effecting any permanent reduction of the supply. During the years 1887 to 1890 there was an alarming scarcity of sardines on the French coast, and the outlook for the industry was serious, but after four years the fish returned in their former numbers. The history of the sardine fishery shows what extensive operations may be supported annually when the natural conditions permit the fish to spawn unmolested, the spawning grounds in this case being many miles offshore.

Several American fishes resemble the pilchard, among them the sea herring and the California sardine. The former is extensively canned on the coast of Maine, and often placed on the market as 'genuine French sardines in pure olive oil'; the latter is canned to a limited extent in southern California.

The sardine fishery of France dates back many years, and even in the early part of the eighteenth century was an important industry, but it has become much more extensive since the introduction of canning. The building of railroads has also benefited the fishery by providing means of shipping to the inland points that part of the catch which can not be disposed of locally.

SARDINE FISHERMAN'S HOUSE, BRITTANY.

The province of Brittany supports by far the most productive fisheries and is the center of the canning industry. Here in 1898 were 21,684 fishermen, with 4,611 boats, and here were caught 49,478,365 kilograms of sardines, selling at 7,572,347 francs. The leading center is Douarnenez, which is credited with 4,200 fisherman, 710 boats, and over 18,000,000 kilograms of sardines, valued at 2,442,000 francs. Next in importance is Concarneau, with 2,695 fisherman, 490 boats, and 9,163,000 kilograms of sardines, worth 1,719,890 francs. Other important places in Brittany are Audierne, Quimper, Port Louis, Etel, Quiberon, La Turballe and Le Croisic. Outside of Brittany the fishery is most extensive at Sables-d'Olonne, St. Gilles-sur-Vie and Arcachon. On the Mediterranean coast of France sardines are caught at numerous places and by many fisherman, but only in relatively small quantities. The fisheries here in 1898 gave employment to 7,794 men, using 2,861 boats, the catch being 2,129,519 kilograms, valued at 987,738 francs.

Formerly in parts of Brittany nets were used to surround the schools and then stones were thrown in to frighten the fish into the

meshes. In this way large catches were often made and the market was glutted; but the method came into disrepute and is no longer followed. Fishing is now carried on exclusively with gill nets, made of very fine cotton twine; they are 45 yards long and 500 meshes deep, and are kept in position in the water by numerous cork floats and a few stone sinkers. The mesh is necessarily very small, as it is intended to gill the tiny sardines. The nets vary in fineness to suit the different runs of sardines, and are of about three standard sizes. The largest mesh is equal, in America, to 0.66 inch, bar measure, while the smallest size equals 0.40 inch. The complement of each boat is 10 nets, representing the three sizes of mesh.

The nets are dyed a bright greenish blue, and when suspended from the masts to dry add to the picturesqueness of the fishing boats and the wharf scenes. The dyeing is for the twofold purpose of preserving the nets and rendering them less conspicuous when in the water.

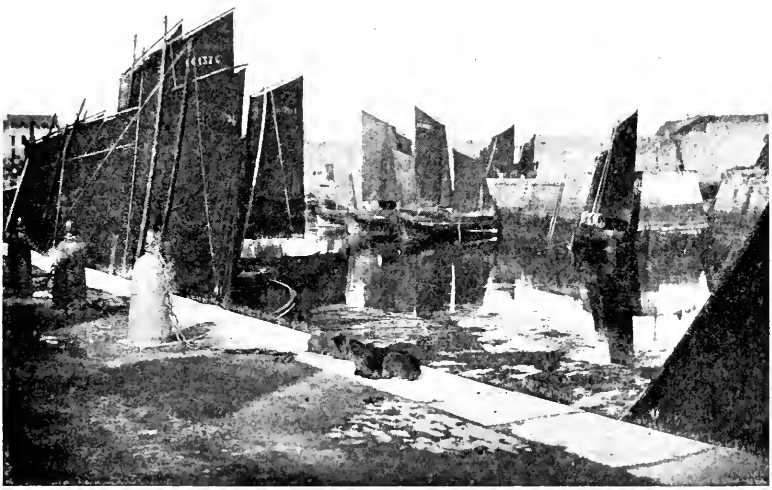
In the fishery for sardines for canning, bait is almost as important as the boats and nets. In no other net fishery in the world is bait so extensively employed and so essential to the success of the industry. The scarcity of bait is always a serious matter in the fishing districts, curtailing the catch, reducing the income of the fisherman, and often producing distress among the fisherfolk. It is therefore remarkable that for this indispensable article the French should be absolutely dependent on other countries and that the success of the fishery for sardines should be intimately related to the fisheries for other species in distant lands.

In the early days of the sardine fishery, especially prior to the establishment of canneries, small shrimp-like animals, about half an inch in length, were much used as bait. The gathering of this kind of bait was an occupation of the women, who sought the schools in the bays and coves, catching them in large canvas bag-nets. They frequently made their best catches in water up to their necks, when the weather was bad and the water along the shores was thick. The taking of these little creatures appears to have been prohibited many years ago, because of the supposed destruction of the eggs at the time of catching the shrimps. Although the interdiction is now removed, little effort is made to secure this form of bait.

The bait now in general use is the salted eggs of the cod, though the eggs of hake, haddock, pollock, cusk, herring, mackerel and many other fishes are also employed. Cod eggs are not known to possess any properties which make them superior to the eggs of several other species, but owe their prominence to the abundance of cod in regions on which the sardine fishermen depend for their bait supply. The annual consumption of roe in France at present is 40,000 to 45,000 barrels, for which the fishermen pay about $300,000. It is reported that in favor-

able seasons as many as 25,000 barrels of roe have been expended in Concarneau alone.

For at least two centuries cod roe has been imported from Norway, which country has always furnished the greater part of the sardine bait. Other countries which have contributed supplies are Holland, Newfoundland and the United States. From time to time the French Government has encouraged its own cod fishermen (at St. Pierre and Miquelon; on the Grand Banks; in the waters of Iceland, and in the North Sea) to preserve the roes of cod and other fish, and in 1816 offered a bounty of $4.00 a barrel for roe made from fish caught by them; but this and other inducements have had little effect on the supply from native sources.



Sardine Boats in the Harbor of Concarneau.

The price of roe has varied greatly from year to year. In the early part of the eighteenth century, bait was bought for 50 cents to $1.00 a barrel, and throughout that century prices were comparatively low. In the second decade of the last century prices reached their highest point; they were apparently never less than $32.00, and ranged from that to $60.00 per barrel. By 1822 the price had fallen as low as $5.00 or $6.00, and since then has seldom been as high as $25.00 or $26.00, averaging $12.00 or $15.00 The average price for Norwegian roe recently has been about $7.00 per barrel. In 1900, owing to the failure of the Norwegian cod fishery and the resulting scarcity of roe, the price for Norwegian bait rose to $24.00 per barrel. The price of

American and Newfoundland roe is but little more than half that of Norwegian. In 1900 the best American roe was selling at $8.60 a barrel and in the previous year at only $4.60.

The sardine fishermen use peanut meal or flour to mix with the roe, it being much cheaper. Floating lightly and being quite conspicuous, it attracts the attention of the sardines, which readily devour it.

In the Mediterranean sardines are caught during every month of the year. On the west coast, however, the fishing season opens in February and continues to November, rarely extending into December. Fishing in the canning districts is continued as late as practicable, usually as long as the fish remain in abundance, as their condition at that time is good.

The sardine fishery is emphatically a shore fishery, and most of it is done within a very short distance of the home ports. This permits the use of smaller and less expensive boats than would otherwise be required, and insures the landing of the fish a short time after capture. The early fishing for the *sardines de derive* is mostly within 1 or 2 miles of the shore and rarely beyond 5 or 6 miles. In the summer and fall fishing with bait, the boats may go 10 miles to sea, but the largest part of the catch is taken within 3 or 4 miles of shore, and a very considerable proportion close inshore in the bays.

The fishing in the early part of the season— that is, in March, April and May—is done mostly with old nets and is conducted only at night. While the boats are lying near by and the men sleeping, the nets are allowed to drift. No bait is used. The fish thus caught are not fat and are not used for canning, but are salted or sold for immediate consumption. The regular fishing is carried on only by day. The boats start for the fishing-grounds early in the morning (2 to 4 o'clock), so as to be there when day breaks. They may also have to leave earlier if the tide would otherwise beach them. The best fishing is in the early morning, and the boats are often back to port by 9 or 10 o'clock with full fares.

When a boat arrives on the fishing-grounds, a net is shot and slowly towed by means of a short line attached to the cork line and fastened in the stern of the boat. In summer fishing, when sardines are abundant, the fishermen often let one net go adrift when it is full of fish, trusting to pick it up later, and put out another net. Indeed, a boat may have fish in three nets at one time, though this is rarely the case.

Bait is always used in the day fishing, being necessary in order to attract the fish to the vicinity of the boats and into the nets. The casting of the bait, on the proper use of which a great deal of the success of fishing depends, is always done by the master or 'patron,' who stands in the stern of the boat on a little platform and uses the flour and roe

Sardine Boats sailing for the Fishing-grounds.

A Glimpse of the Concarneau Sardine Fleet, discharging the Day's Catch.

as required. When the fish have come toward the surface and are on one side or the other of the net, his object is to cast the bait in such a way that they will rush against the net and become gilled.

Considerable skill and experience are of course necessary in managing the net and in having it hang properly in the water and not become folded or wavy owing to currents or tide. Unless the net is straight or gently curved, the fish will see and avoid it. When a net contains fish and is ready for hauling, it is taken in the boat and the fish are removed from the meshes by gently shaking the net.

The sardines are often found in a compact body, and the boats will be concentrated in a comparatively small area, at times so close together that the operation of the net would seem almost impossible and the chance of catching fish very improbable. The entire fleet of a given

FISHING BOATS ON THE SHORE, CONCARNEAU.

port—consisting of several hundred boats—may be at work on one school and fishing literally *en masse* instead of individually.

No ice or other preservative is used on the fish, which are landed a short time after gilling. The fish reach port in good condition, and are often at the canneries within one or two hours after capture. Should the failure or unfavorable direction of the wind threaten to delay the arrival of the boats, and hence impair the quality of the fish, the crews row leisurely back to the port.

Soon after reaching port the nets are spread for drying, being hauled to the top of the masts and suspended between them for this purpose. When all the fleet has arrived and the nets are spread, the view of the maze of blue nets, sails and masts is most interesting and unique.

When the fishing boats begin to arrive, the wharves, which have practically been deserted, assume a very busy and animated appearance, and as the arrivals increase in number the bustle among the different classes of people becomes intense, although good nature and good order prevail. The foreign visitor here witnesses some exceedingly interesting and picturesque fishing scenes—thousands of fishermen in their coarse blouses and flat cloth caps, with trousers rolled up and their feet bare or in the huge wooden shoes of the country, unloading their fish and carrying them to the canneries; hundreds of women and girls in short dark skirts, white caps and collars, and wooden shoes, negotiating for sardines, receiving the fish from the fishermen, and dispatching them to the canneries; sardine boats, either rowed or sailed, entering the harbor in groups or singly and coming up to the already congested docks; fish wagons going to and from the factories, and a mixed crowd of merchants, sight-seers, artists and idlers. The commingled noise of waves, boats, wagons and tongues is underlain by the incessant rattle of wooden shoes on the stony pavements.

The prices received by the fishermen are regulated by the factory operators, and depend on the supply, the size and quality of the fish, the weather and other considerations. The fish of each boat are virtually sold at auction, only there is as a rule no counter bidding, the prices offered by one or two factories being adopted by the others and accepted by the fishermen. If a fisherman is not satisfied with the price offered by one factory, he is at liberty to seek a higher price elsewhere. Some boats always sell their catch to the same factory, and all of them, to a greater or less extent, deal with particular factories. The maximum price which factory operators can profitably pay for sardines is $5.00 per 1,000 fish. The dealers in fresh sardines can pay as much as $7.00 per 1,000. At times the demand for sardines to be sold fresh *(au vert)* tends to keep up the prices; but this use is limited and does not interfere greatly with the cannery demands.

Women usually represent the factories as purchasing agents. They are given considerable discretion by their employers and are very sharp in making bargains. Payments are not made in money, but in tokens or tickets which are redeemed weekly. As the fishermen deliver their fish, two baskets full at a time, to the agents of the canneries, they receive a metal tag or token with the name of the buyer on it. When all the fish are landed the metal pieces are counted and surrendered, and a claim check is issued in their place. At the end of each week the master or the owner of the boat (sometimes the same person) goes to the factory, receives the money due, and apportions the earnings of the crew.

The division of the proceeds of fishing is rather complicated. The boat, nets, equipment and bait usually belong to a nonfisherman (who

may own a number of boats). The men of the crew furnish their own food, fuel and clothing. The owner is entitled to half the sales of fish, and the remainder goes to the crew in the following proportions: There being 6 men in the crew, 4 of them get equal parts, the captain receives the share of one man plus 10 per cent. and the cook half a share. Dividing the proceeds into 22 parts, the boat owner is entitled to 11 parts, 4 members of the crew to 8 parts, the master to 2 parts and the cook to 1 part; the share of the master being increased by 10 per cent. of 2 parts and that of each member of the crew diminished by 2½ per cent.

From the time the men begin to fish until the close of the season, they pay to the government 1.10 francs per month, in consideration of which they are pensioned on attaining the age of 50, provided they have served 300 months on sea duty (either in fishing or in any other maritime occupation). They also pay 1.50 francs per month as premium on an insurance fund which the government allows for injury due to the vicissitudes of sea life. In case of death, the family of the fisherman receives an annual pension depending on the size of the family and on age and length of sea service of the deceased, the minimum sum being 300 francs; naval service increases the pension.

The average stock per boat in a given season varies greatly on different parts of the French coast, depending on various local causes besides the abundance of fish, such as weather, bait supply, local demand, shipping facilities, energy with which fishing is prosecuted and other evident factors. The boats fishing out of Brittany ports have a larger average yield than those of other ports of the west coast; and those in the Mediterranean have by far the smallest stocks. Thus, in 1898, the average catch per boat was about 10,700 kilograms of sardines in Brittany, 3,300 kilograms in the southern part of the Bay of Biscay and only 745 kilograms in the Mediterranean.

The construction of the first sardine canning establishment dates from about 1845, since which time the growth of the business has been almost uninterrupted. The factories gave to the sardine fishery a great impetus, and to-day are the chief supporters of the very extensive fishing operations in the Bay of Biscay. They employ many thousand persons, at what are considered good wages, and in some of the fishing towns give work to practically all able-bodied persons who are not engaged in fishing. In Concarneau, a town of 10,000 people, fully 3,000 men, women and children are directly connected with the sardine canning business, besides the fishermen. Most of the work in connection with the canning of sardines is done by women and girls, a few men being employed for special duties for which women are not adapted. The factories are generally large stone structures surrounded by a stone wall and inclosing a courtyard. Some are able to utilize

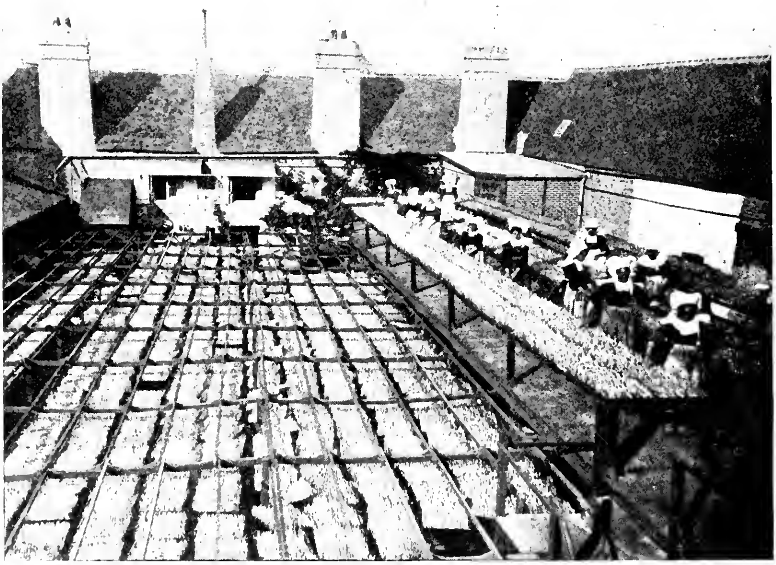
upward of a quarter of a million of fish daily. The yearly output of individual establishments is from 300,000 to 4,000,000 or 5,000,000 boxes. No complete statistics for the canning industry are available, but over 100 factories are operated and not less than 15,000 persons are employed therein. Concarneau and Douarnenez have more factories than any other localities, the number operated in 1900 being 29 and 25, respectively. A large number of the canning establishments are owned or leased by companies having headquarters at Bordeaux and Nantes.

The various processes to which the sardines are subjected in the course of canning may now briefly be noticed. As soon as the fish reach the factories, their heads and viscera are removed by women, who perform their work with great rapidity. The fish are then sorted by size into large tubs of strong brine, where they remain for about an hour. They are then placed in small wicker baskets and washed in either fresh or salt water for a few seconds, to remove loose scales, dirt and undissolved salt.

Drying, the next step, is done preferably in the open air, and a large part of the product is so treated. For open-air drying the fish are arranged by hand, one by one, in wire baskets or trays, holding about 150 fish of medium size, placed on wooden frames or flakes. The distinctive feature of the trays is their division into about 7 V-shaped crosswise compartments, in which the sardines are placed in regular rows, with their tails upward, so as to promote the escape of water from the abdominal cavity. The sardines remain out for a variable time, depending on their size, the state of the atmosphere, etc. The usual time in favorable weather is one hour. In damp, foggy or rainy weather the sardines must be dried indoors by artificial heat, and drying ensues much sooner than in the open air. Some factories, not being provided with driers, are unable to operate in such weather. In most of the factories, especially those more recently constructed, artificial heat is supplied in a special drying chamber by means of steam pipes.

From the drying flakes the fish are taken in the same wire baskets to the cooking room and immersed in boiling oil, in open vats of various sizes and construction. As the fish are quite dry, much of the oil is taken up in cooking and has to be replaced from time to time by fresh oil. The immersion in oil usually lasts about two minutes, but varies with the size of the fish and is best gauged by experience. The baskets are first removed to a table or platform with an inclined metal top, where the surplus oil is allowed to drain from the fish, and then taken to the packing room. There the sardines are carefully placed in tin cans. After the cans are sealed, they are immersed in boiling water for several hours; this accomplishes a fourfold purpose: (1) The

GROUP OF EMPLOYEES IN YARD OF A CANNERY, BRITTANY.



SARDINES DRYING ON GRILLS IN YARD OF CANNERY.

cooking of the fish is completed; (2) the bones are softened; (3) the bacteria in the oil and fish are killed; (4) the presence of leaks in the cans is disclosed. After cooling, the cans are placed in dry sawdust and stirred from time to time; this absorbs the oil and moisture on the surface, and renders the cans clean and ready for packing.

The sardine manufacturers ostensibly employ only two kinds of oil in their canning operations—olive oil and arachide or peanut oil. Native olive oil is used with the best quality of sardines. Fish packed in it will remain in good condition ten years or longer, and are reported to be better the second year after packing than earlier. Arachide oil is extensively employed. It is made in Bordeaux, Fecamp and Marseilles from peanuts imported from India, Senegal and other parts of Africa, and other countries. It comes in three grades, the best quality costing less than one-third as much as the best olive oil. Peanut oil is largely used to meet the American demand for a low-priced sardine. Most of the cheaper French sardines exported to America are packed in peanut oil, which is practically tasteless. While it is reported that the manufacturers knowingly handle only the oils named, it is understood that cottonseed oil, being tasteless and cheap, is used by the French oil-dealers for adulterating both olive and peanut oils. A canner may fry his sardines in peanut oil and fill the cans with olive oil, or vice versa; or one oil, with or without the admixture of cottonseed oil, may be used throughout the process.

There are various other ingredients with which or in which the sardines are packed to give them flavor or piquancy. Some of the very best goods are prepared with melted butter instead of oil; these are mostly for special French trade. Tomato sauce, pickles and truffles are also used. With most of the oil sardines a small quantity of spices is added in order to impart a flavor. The usual ingredients for each can are 1 or 2 cloves, quarter or half of a laurel leaf, and a small piece of thyme; these are put in the can before the fish, so that they will be on top when the can is opened. The fresh leaves of tarragon are sometimes used.

Americans need hardly be told that French sardines, when of the best quality, have a flavor and richness which make them preferable to the sardines prepared on the Atlantic coast of the United States from the young of the sea herring. French sardines of average grade, even when canned in peanut and cottonseed oil, are superior in palatability to the great bulk of the American output; while the cheaper grades of French sardines—which unfortunately find a ready market in the United States—are certainly not preferable to much of the native pack.

The conditions which underlie the general superiority of the French canned sardines, and the steps which may be followed in America for

narrowing the gap which now separates the products of the two countries, appear to the writer to be chiefly as follows: (1) The methods adopted in the French sardine fishery result in the landing of the fish in excellent condition. This is the main object and is never lost sight of. The fish are caught singly in a delicate mesh, removed by hand, carefully kept on board the boats so as to avoid crowding and mashing, counted by hand into small baskets, taken to the factories within a few hours after being caught, and promptly put through the preserving process, so that ordinarily the deterioration which ensues is not worthy of mention. (2) In France the sardines caught in the early part of the season are not canned, because they are not in the best condition. It is only after the fish have become fat that they are considered suitable for canning. The fattening depends on an abundance of proper food, and along with it is an improvement in the flavor and general quality of the flesh.

While the young sea herring is an excellent fish, it may be admitted that even when at its best its meat is inferior to that of the fat young pilchard in richness. The latter has a peculiar flavor which, to a considerable degree, is preserved in canning and which probably can not be successfully imitated in the sea herring. However, the difference in flavor between the French and the American sardines on which many persons lay much stress appears to the writer to be of only secondary importance. The taste for French sardines has been acquired and perpetuated in the United States because of the long-continued unsatisfactory quality of American sardines. The herring is naturally no less wholesome than the pilchard. If it is caught for canning only when in prime condition, and if, in the form of canned sardines, it is placed on the markets with the minimum amount of deterioration and with such adjuvants in the way of oil, spices, etc., as may be suitable, it should and will receive ample recognition at home, and meet with a constantly increasing demand at prices that are now hardly dreamed of.

The history of a few canneries on our east coast during recent years has shown that a very marked improvement in the quality of American sardines is entirely practicable, and, furthermore, is highly appreciated by consumers, as evidenced by the much higher prices they are willing to pay and the steady demand beyond the capacity of the factories. With regard to the sardines of the Pacific coast of the United States, there is no reason why they should not, when properly canned, prove equal to the French fish in every respect. The high reputation which has been acquired by the comparatively small quantities packed in California during the past five or six years, and the excellent prices which they have commanded, argue well for the success of an extensive business.

THE LATE EPIDEMIC OF SMALLPOX IN THE UNITED STATES.

By Dr. JAMES NEVINS HYDE,

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THE adaptability of man to his environment is one of many generous provisions for his welfare. But it is a provision with conditions. The adaptation once secure, even a temporary failure of complete adjustment to the environment may be perilous.

The commercial travelers of all countries are accounted, on the whole, as of a healthy class; they breathe all airs, they drink all waters, they consume all foods with impunity. They are rarely adjusted to a single environment for any length of time. The farmer, on the other hand, long habituated to his narrow circle of surroundings, would often become seriously ill if for a time he should leave his farm and village to breathe the air and drink the water and consume the food that are familiars of the traveling salesman who would sell a lightning-rod for the protection of the farmhouse.

This adaptability extends to a surprising degree toward the limit of endurance of toxic agencies. The farmer whose case has been supposed may year after year drink with impunity the water from a well contaminated with germs that would promptly induce typhoid fever in one wholly unaccustomed to a daily dosage of the poison. But the same farmer may lose his immunity if for any length of time he removes to another residence and afterward, returning to his own place, makes use of the contaminated water to which he was once habituated.

The greatest peril from loss of adaptation to environment lies in the changes wrought by the sudden removal of a man from his country home, or even from a less salubrious city residence, to a situation where men are massed together in considerable number. Here a new and complex problem is presented. If every man of those thus suddenly congregated had recently surrendered his adaptation to a special environment, the chances of thus begetting disease are enormously multiplied. Such a condition is presented in prisons, hospitals, great fairs (such as those at Nijni-Novgorod, Chicago and Paris), and especially in the camps of soldiers. The camp as a focus of disease is more potent than all others; for one reason, among others, that even though previously subjected to selection by physical examination, and supposedly under the direction of sanitarians, the recruits are not free to select for

themselves their sleeping places, food and clothing, but are at many points under subjection. Not only are they densely massed together, but they are not adapted to the new environment, even after they become veterans, who may be classed as respects immunity with the commercial traveler and the 'globe-trotter.'

War and pestilence are twin brothers, but they do not always work side by side. Often pestilence follows war; more rarely they reap their dreadful harvest on the same day. The word 'pestilence' should be understood to include not merely the grave plagues that have decimated the human race, but the less severe epidemics of disease which have spread over large areas of space and affected to a less extent great numbers of the human family. Even thus, however, in comparison, the deadliness of war is far surpassed by its grim camp-follower. Where the one slays its thousands, the other destroys its ten thousands.

In this country, the epidemic visitations following war have been both mitigated and severe. We fought Great Britain in the Revolution, and soon after were afflicted with maladies some of which had not before tormented our people. Soon after 1780 the daily papers of Boston, New York and Philadelphia were filled with advertisements of remedies for the itch, a malady which had never before so multiplied on our soil, water being abundant, soap cheap and the habits of our forefathers cleanly. The War of 1812 was chiefly naval and its aftermath of disease insignificant, for the reason that of all afloat the American war vessel has ever been the most scrupulously clean. But the Mexican War was followed by an epidemic of cholera of severe grade; and the late Civil War was the precursor of a succession of typho-malarial fevers that were previously almost unknown save in certain special localities and to physicians there resident. In a similar way the plague followed the Saracen armies under Mahomet in 622; syphilis spread through Europe after the campaign of the dissolute Frenchmen who followed to Italy the standard of Charles VIII.; and the English paid a price for the crushing of the last of the Plantagenets on Bosworth field in the epidemic of 'sweating sickness' that ensued.

Our late war with Spain was followed by an epidemic disorder which spread extensively throughout the United States, and which has attracted but little attention from our public economists, for the reason that it has been suggested to few to see the results in a comprehensive survey of the broad area involved in the extension of the disease. The malady spread from the eastern and southern borders of the United States to the Middle West, and thence in regular progression to the Pacific Slope, including in its progress not merely the States where there are efficient health boards, possessing ample powers and trained officials, operating with modern methods, such as New York, Pennsylvania, Ohio and Illinois; but also the as yet partially

settled districts of the further West as far as Idaho, Oregon and California. The sweep of the malady has included individuals of the white race,.the Indians and the negroes, the well-to-do and the poor, the filthy and the cleanly, people of all sorts and conditions. If the results had been in any considerable proportion grave, the entire country would have been alarmed, and the attention of all classes concentrated with a profound interest upon the earliest invasion and progress of the disease in the several localities where it spread. But, fortunately, the results were mild, so mild, indeed, that the nature of the epidemic, certainly at first, was misunderstood in almost all the places where its victims were discovered. Medical men, well trained in their profession, in many cases could not recognize the nature of the malady by reason of the special features it now for the first time presented. Some physicians, even after demonstration by experts of the character of the symptoms before their eyes, refused to accept the inevitable conclusions. Though obviously a contagious disease and one spreading in epidemic form in an astonishingly large number of villages and towns, East and West, the victims of the disease, because of the very general misapprehension respecting its nature, were permitted free access to those not affected. In many such centers of population, persons betraying all the external evidences of the disease attended churches, schools and theaters; delivered milk, groceries and other provisions at the houses of their customers; officiated in public stations; and even slept in beds occupied by other non-infected members of the same family. A study of the special character of this epidemic possesses interest, because, as a matter of fact, the malady was smallpox.

The history of smallpox in classical career has been studied with a patient faithfulness and with an attention to every detail that is set forth fully in most of the text-books. Few trained physicians are ignorant of the essential facts thus collated. In the late epidemic visiting this country, confusion in many cases arose from the total failure of the symptoms of the disease to correspond with the classical types previously portrayed in the books and encountered in practice. Almost all the histories of smallpox in the past have been descriptive of epidemics that spread among a people either previously unprotected from the disease by modern methods, or through the medium of individuals not so protected. It might, however, have been expected that an epidemic of disease occurring during the last century and another at the beginning of the present, operating on a different soil and under different conditions, would exhibit differences in type.

That smallpox may be so modified as to be stripped of every one of its formidable features has long been known. The so-called *variola sine variolis* (smallpox without pocks) is not a fiction of the schools, but a fact of experience. In these instances, after a day or two in

which there may be slight sensations of chilliness and possibly moderate fever, the disease actually not preventing the patient from attending to his or her usual vocation, the end is reached, and without the occurrence of eruptive symptoms. These cases are sufficiently common, and the proof of the reality of the variolous process in one class, where the patient afterward is not capable of receiving disease in an epidemic, is substantiated by the proofs furnished by another class of cases, in which, for example, a pregnant woman, having suffered no more than in the instance cited, later brings into the world a child covered with unmistakable symptoms of the disease.

From the extreme of benignancy illustrated in such a group of cases to another in which symptoms are exhibited of a severity just short of the pronounced features of classical smallpox, there is every gradation and not a few excursions to the one side or the other of oddity and apparent caprice. In the late epidemic, physicians were often at sea respecting the nature of the disease, because, perhaps, after a regular onset of classical and threatening symptoms, there followed an almost absurd abortion of the morbid process, which in twenty-four hours or more lost every menacing feature; or the eruptive phenomena failed to develop the characteristic fluting or puckering of the vesico-pustules known technically as 'umbilication'; or the peculiar odor of the disease was lacking; or the mouth failed to exhibit symptoms; or the progression of the eruptive phenomena from point to point of the body-surface was not according to rule.

The question of the influence of vaccination upon the victims of the epidemic and others aroused special interest. It was claimed in many of the localities where the disease prevailed that the vaccinated and unvaccinated suffered alike; and hence that vaccination did not protect. It was further claimed that in some cases vaccination had been effective in those who were convalescent from the new disease. And thus blunders innumerable complicated the question, the answer to which was of the highest moment to the welfare of the commonwealth. The disease was variously called 'Cuban itch,' 'Porto Rico scratches,' 'Cuban measles,' 'chicken-pox,' 'Porto Rican chicken-pox,' 'Spanish measels,' etc. These popular names constituted the jargon of the ignorant. There are no maladies in Cuba, Porto Rico or Spain recognized by any such terms or others like them.

Greed is among the most potent of human motives, and it must be admitted that in the presence of the late epidemic, among those who were ignorant of its nature, there were to be found others who preferred to close their eyes to the facts. Merchants did not care to suffer the paralysis of their local trade which usually is wrought by the panic that flees before a pestilence. Editors of papers in the smaller towns were unwilling to spread the news to their immediate rivals in the

adjacent county that their readers were victims of a disease which should be fought by quarantine. School boards did not care to dislocate the machinery of their system. Manufacturers pleaded for the families likely to be ruined if their works were shut down. In the minds of many there were a shame and a disgrace associated with the fact that they were singled out for the explosions of the pest; and there was some reason for this. Hence, in a considerable proportion of cases, the officers of local government, the large employers of labor, school superintendents and others refused to accept the facts, basing their belief on the evident mildness of the malady, and often upon the remarkable result that after a fortnight or more of the prevalence of the disease in their community there had been either no fatal results or so few that in several hundreds of cases the disproportionate mortality was so small as to disprove the accusation that smallpox was prevalent.

And yet smallpox indeed it was; mitigated, it is true, but still capable of awaking to a frightful activity in a favorable field and at an opportune moment. For it is among the facts established by a bitter experience that the mildest and most modified type of the disease, varioloid, for example, of insignificant features, may be the source of one of those epidemics of smallpox which rival in their mortality the most direful of the scourges that have afflicted the race.

Why was the late epidemic the mildest in its type and consequences of any of the same nature that have preceded it? Why were its features so masked that even physicians of experience failed to recognize them? Why was the resulting mortality so slight that the malady awakened little dread in the communities which it invaded, the people, made familiar by contact with its manifestations, failing to exhibit the horror which has usually been excited by its presence?

The answer is inwrought with the solution of some of the tremendous problems of the future of the human race. If devastating plagues cannot be wholly obliterated, can they be so modified by scientific methods that they are gradually converted into trifling ailments, productive of minimized danger and followed by trifling sequels? The culture-tubes and culture-plates of our bacteriological laboratories have spelled out the answer in sterilized media. The potency of almost all germs may be first gradually weakened and later annihilated by cultivation in special soils. Fraenkel has demonstrated that an enduring decrease, even a complete and irrevocable loss of virulence, has been produced by artificial cultivation of most of the different species of pathogenic bacteria, among which may be cited as conspicuous examples the germs of swine-erysipelas, of symptomatic anthrax and of pneumonia. Thus a minute organism, descended from a death-dealing source, may

become in the culture-tubes of the experimenter as harmless as those found in an ordinary infusion of hay, such as the *bacillus subtilis*. Even thus the wild boar is proven the ancestor of the domestic hog, and the wild-cat the remote progenitor of the Angora kitten.

Even scarlet fever, under the impulse of some such causes as those under discussion, has evolved a 'new type,' which has been set forth by a competent health officer, Dr. William Robertson, of Leith. He reports that compulsory notification has not only lowered the mortality of scarlet fever, but actually modified its type, so much so that it is now difficult to tell when one has or has not to deal with the suspected disorder. On every side one hears it repeated that epidemics are now characterized by a want of symptoms and signs. The bright red rash is seldom seen, and when there is a rash it disappears before the arrival of the medical attendant. If one looks for throat-signs, they, too, may have been transitory. The symptoms of onset are so slight that even an anxious parent takes no notice of a passing indisposition.

These are the evidences, oftentimes somewhat vague, but again both significant and unmistakable, that the dream of the scientist is to have its realization in the future. Few believe that the great pests of the human family will be suddenly jugulated or annihilated. The gradual extinction of each by modification, by attenuation of virus, and by elimination of grave symptoms, is the aim of scientific medicine, and its disciples can thank God and take courage for the fruits of their labor, realized each year in larger measure and with fuller promise.

The germ of all epidemics of smallpox is one, but the soils on which it has grown are many. The culture-tubes and culture-plates on which it has been propagated until it has lost much of its potency and even many of its features are the bodies of the men and women of the last quarter of the nineteenth and the early part of the twentieth centuries.

The late epidemic of smallpox in the United States was the legitimate fruit of the Spanish-American War, and the popular terms by which it was designated among the common people, like almost all folk-words, contained a kernel of truth. Cuba and Porto Rico, before our armies descended upon their shores, were like the Philippine Islands, very abiding-places and citadels of smallpox. Our returning troops brought back with them the effective elements which lighted up the late epidemic in the United States. But the germ-carriers in this instance were our own previously vaccinated soldiers. The germ was attenuated in its potency at the outset. When it gathered to itself the added power by which it was enabled to spread from community to community, its extension was not through a population virgin of protection by previous vaccination, but for the most part constituted either of the vaccinated or of the children of the vaccinated.

Unvaccinated but yet 'children of the vaccinated'—is any degree of immunity conferred by inheritance? However difficult of exact demonstration, the affirmative must be accepted not merely as a logical sequence of the experiments in the laboratory to which reference has been made, but by certain clinical phenomena of striking importance. For example, it is well known that among some of the immigrants touching our shores for the first time, who come from countries where the mosquito is not found, notably from English homes, the ravages produced in midsummer, when women and children especially are lodged in cheap boarding-houses, with windows unprotected by screens, the results of the attacks of the American insect upon their exposed skins are of a grade of severity unparalleled among natives of our soil. Generations of Americans have succeeded in establishing a partial immunity by the mere succession of these accidents in a long series of summers; so that while they may, and actually do, suffer from mosquito bites, the effects are far milder and without any proportion to those experienced by the immigrant. A striking illustration of this fact is recorded in the history of the Revolutionary War, when in the midst of their first summer on this soil the mercenary troops from Hesse-Darmstadt and Hesse-Cassel were so savagely attacked on their march from Trenton that whole platoons of troops were unable to distinguish objects through their swollen eye-lids, and were thus rendered wholly unfit for duty. Looking at the obverse of this proposition, every student of public hygiene is aware of the fact that truly formidable ravages of smallpox occur in epidemics attacking virgin populations, as, for example, islanders long unvisited by Europeans, where neither the individuals themselves nor their ancestors for generations have enjoyed the immunizing protection of vaccination. In these cases it is often not merely a decimation which results, but it may be a destruction of more than half of the entire population. In a few isolated instances almost every individual of a tribe or village has been cut off. Not the sins alone of the fathers but some of their safeguards are visited upon the children. The clean living that drove away leprosy from English soil and that so widely substituted the gout for the 'King's evil,' has tinctured the blood of the children of the men who fought at Naseby and learned a lesson in humanity from Howard.

It will be seen that the whole question pivots upon vaccination. It is necessary to look critically upon this means of securing immunity, for the procedure is again under the searchlight.

All said and done, vaccination is an invaluable means of securing immunity against smallpox, but it is not a perfect means. What artificial conquests of man are rounded to the perfection-point? Every one knows that the finest double-screw steel vessel that steams across the Atlantic can be crushed by a single blow of the arm of the sea if

the gale be sufficiently furious and the billows sufficiently huge. As it is necessary to admit that even one attack of smallpox does not confer absolute immunity against a second, seeing that some men have had two and even more of such attacks in a lifetime, equally must it be admitted that vaccinated persons, and even many times re-vaccinated persons, have had attacks of smallpox. In ordinary seasons when smallpox is not prevalent a larger protection is conferred by vaccination than that conferred by the lightning-rod upon the dweller beneath the roof above which it rises. But in seasons of epidemic influence, even though the epidemic be as mild as that which furnishes the theme of this paper, such an influence is appreciable and in many cases highly effective. At these times those who previously were incapable of being vaccinated (even the vaccini culturists occasionally find heifers which cannot be made to serve as vaccinifers) are inoculated with ease; at such times also vaccination can be made effective even after the onset of unmistakable symptoms of smallpox; at such times also even those who have had smallpox can be vaccinated, and that after the recent establishment of convalescence. The ardent advocates of the position that the mild epidemic through which this country has just passed was not one of smallpox pointed with what seemed to them convincing force to the fact that they had successfully vaccinated the victims of the disease immediately after recovery. But the argument was without force. The skin of a person convalescent from smallpox is in an exceedingly irritable state and readily is excited to the production of local symptoms at the point where the needle of the vaccinator has been at play. It has to be borne in mind that the actual introduction of a disease by inoculation is a far more rigid test than mere exposure to a volatile poison, the kind of exposure through which, as a rule, smallpox is acquired. What physician, for example, would dare to inoculate a patient with the virus of smallpox after the most classically perfect vaccination? He would be held criminally liable for the result if, as might happen, in this way he should disseminate the disease throughout the community in which he lived.

But the spurious results cited of vaccination of convalescents from modified smallpox prove nothing. Even highly typical results would not disprove the fact of a previous attack of modified or unmodified smallpox. It has been shown that both the vaccination process and the variolous process may pursue their career at one and the same time in the same body. But Dr. Mosely, of Kentucky, put the question to a decisive test last year when he vaccinated three negroes, each convalescent from smallpox, immediately on his release from quarantine. Each subsequently exhibited classical results of successful vaccination in due time and course.

Respecting the enormous value of vaccination to the human race, it

would seem scarcely necessary to appeal to statistics at this late hour of the scientific day. But surely so long as we have the poor with us, we shall have with them a class of men whose minds are so curiously constituted that they will select for study the nether side of the social fabric, the weakness of the best of governments, and the minor defects in the character of the world's heroes.

Even as late as the month of April in the first year of the new century one of the largest and most widely read of the daily papers of the country published over the name of a well-known anti-vaccinationist a statement apparently made in good faith to the effect that vaccination counted more victims than smallpox, and that the practice was a relic of barbarism, asking that a halt be called upon the passage of compulsory laws looking to the protection of the people by any such measures. These singular protests against the operation of the most beneficent of life-saving devices will probably be repeated so long as there is a law on any statute book. Their starveling and distorted figures, garnered from the refuse heaps of mortality, must ever and again furnish forth the tables on which these purblind reasoners rely. They close their eyes to the latest signal victory of science in this field. The Island of Porto Rico, according to the report of Surgeon-General Hoff, in the year 1896 harbored no fewer than three thousand cases of smallpox. Imagine a State of the Union of similar size exposed to such an extent to the ravages of the disease! After the establishment, however, of a government vaccine-farm, "eight hundred thousand natives were vaccinated, at a cost of about four cents for each individual, with the result that by October, 1899, no case of smallpox was known either to the military or civil authorities anywhere in the island." This was a fine illustration of the carrying of 'the white man's burden.' Porto Rico bombarded us with a filth-germ and in revenge we made her clean!

In the year 1867 vaccination was made compulsory for school children in the city of Chicago, and for twenty years after there was practical immunity from smallpox for this important class of the population; while the police of the same city, exposed to every form of infectious disease in their surveillance of its several districts, since vaccination was made compulsory also for them, have never developed a case of the disease.

Dr. Buchanan, medical officer of the local government board (England), in 1881 prepared a table of comparative smallpox death rates among Londoners, vaccinated and unvaccinated respectively, for the fifty-two weeks ending May 29, 1881, calculating that the vaccinated persons of all ages living in London, in the twelve months concerned, were 3,620,000, and the unvaccinated of all ages 190,000 in number. This table reads:

Death rate of people of subjoined ages	Per million of each age of the vaccinated class.	Per million of each age of the unvaccinated class.
All ages	90	3,350
Under twenty years	61	4,520
Under five years	40½	5,950

Statistics of this sort might be piled mountains high; but they mean nothing and they count for nothing with the prejudiced. It is well to remember at times that any agency and influence operating at any one moment upon large masses of human beings of both sexes and of all ages has to bear its percentage of damage and death. The killed on the day of the passing of the funeral cortège of Queen Victoria, the fatality attending ocean and railway travel, even the victims of the awful fire-tragedies lately occurring in Paris and New York, which shocked every reader of the public press, have not deterred men and women of ordinary common sense from going to fairs, sleeping in hotels, or crossing the continent by rail or the ocean steamer. Vaccination of every member of any community, including men, women and, in particular, infants will, without any question, be followed by untoward results in a proportion of cases. The mere statistics of common accidents and ordinary disease account for a large part of the list in the relatively slender catalogue of vaccination accidents. Men, women and children perish annually from the stings of bees, from the bites of flies, from the prick of a pin, and from the accidental impaction of a bit of food in the larynx. Lately a physician reported a disease *not* due to vaccination. An infant was brought by appointment to his office in order to be inoculated, but the physician chanced to be called away from the city, and the date of the trifling operation was postponed for a week. In that week the child developed symptoms of syphilis, which would probably have been laid to the account of the vaccination if the latter had been performed.

It has been said that if the modern tourist could be transported to the streets of London in the eighteenth century, before the general adoption of the practice of vaccination, he would be immensely astonished, not so much by the quaintness of the dress and the speech of the people, by the aspect of their shops, and by the odd-looking vehicles on their streets, as by the extraordinary number of pock-marked faces he would encounter on every hand. Even as early as the year 1778, the officers of foreign troops on American soil wrote back to their countrymen in the old world that the American women were surpassingly beautiful and were very 'seldom pock-marked.' Macaulay, describing the distress in London in 1694, wrote as follows: "That disease over which science has since achieved a succession of glorious and beneficent victories was then the most terrible of all the ministers

of death. The havoc of the plague had been more rapid; but the plague had visited our shores only once or twice within living memory, and the smallpox was always present, filling the churchyards with corpses, tormenting with constant fears all whom it had not yet stricken, leaving on those whose lives it spared the hideous traces of its power, turning the babe into a changeling at which the mother shuddered, and making the eyes and cheeks of the betrothed maid objects of horror to the lover."

At last our English brethren have learned the lesson and learned it well. They have had bitter experience of the devastation which smallpox is capable of working among their kindred, whether in the hovel or in the palace. They have mourned the loss of a gracious sovereign smitten with the pestilence on the very throne of their kingdom. While we may not wish to follow them in all matters, they have set us a worthy example in the patience with which they have buttressed their bulwarks of immunity. The germs of this pestilence are powerless against the army of their humble villagers and peasantry, ranks upon ranks of whom bear upon the arms of each no fewer than four, and often as many as six and eight, simultaneously produced scars of successful inoculation of cow-pox. Vaccination should be the seal on the passport of entrance to the public schools, to the voters' booth, to the box of the juryman, and to every position of duty, privilege, profit or honor in the gift of either the State or the Nation.

FOOD AND LAND TENURE.*

BY EDWARD ATKINSON.

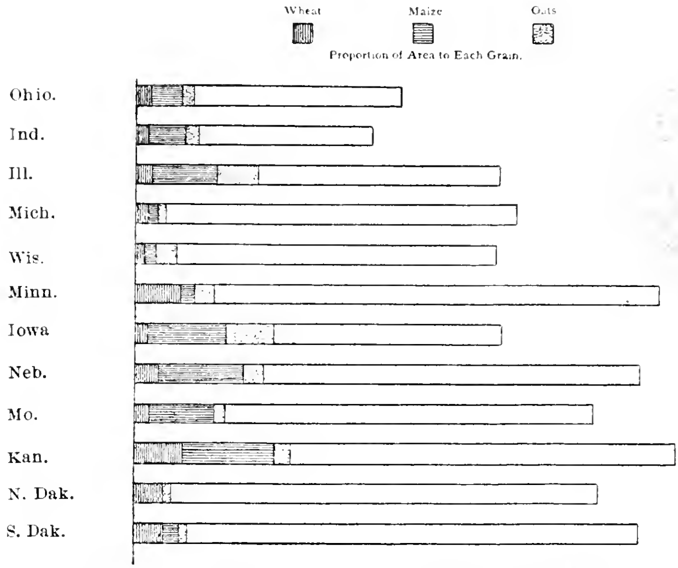
THE conditions of Europe in the present year compel attention to the food supply of what is called the civilized world. The principal supply of grain exported and a large part of the supply of meat are derived from the central United States, in the northern section of the Mississippi Valley.

The area of the twelve States of the northern Mississippi Valley, on which main dependence is placed, is given in the subsequent table; also the proportion of each State which is now devoted to the crops of Indian corn or maize, wheat and oats, which are the chief dependence for the grain supply of man and beast; rye, barley and buckwheat being of minor importance. In another table the proportion of the area of each State which is devoted to each of the three principal grains is indicated graphically.

	Land, Square Mile.	Wheat.	Area Devoted to Maize.	Oats.
Ohio	40,760	2,220	4,514	1,659
Indiana	35,910	1,890	6,299	2,144
Illinois	56,000	2,161	11,156	5,495
Michigan	57,430	1,906	1,688	1,434
Wisconsin	54,450	1,327	1,936	3,026
Minnesota	79,205	7,665	1,505	2,598
Iowa	55,475	2,183	12,577	6,001
Nebraska	76,840	3,229	12,646	2,708
Missouri	48,735	2,356	10,084	1,408
Kansas	81,700	7,282	13,476	2,129
North Dakota	70,195	4,202	37	956
South Dakota	76,850	4,563	1,876	920
	753,550	40,984	77,794	30,478

By far the larger proportion of all these States is arable land, portions of the western section being for the present uncertain in their product, because of semi-arid conditions. Ere long, however, these sections may become the most productive, irrigation on a national scale being under way.

* Read at the meeting of the British Association for the Advancement of Science, Glasgow, September 11-19.



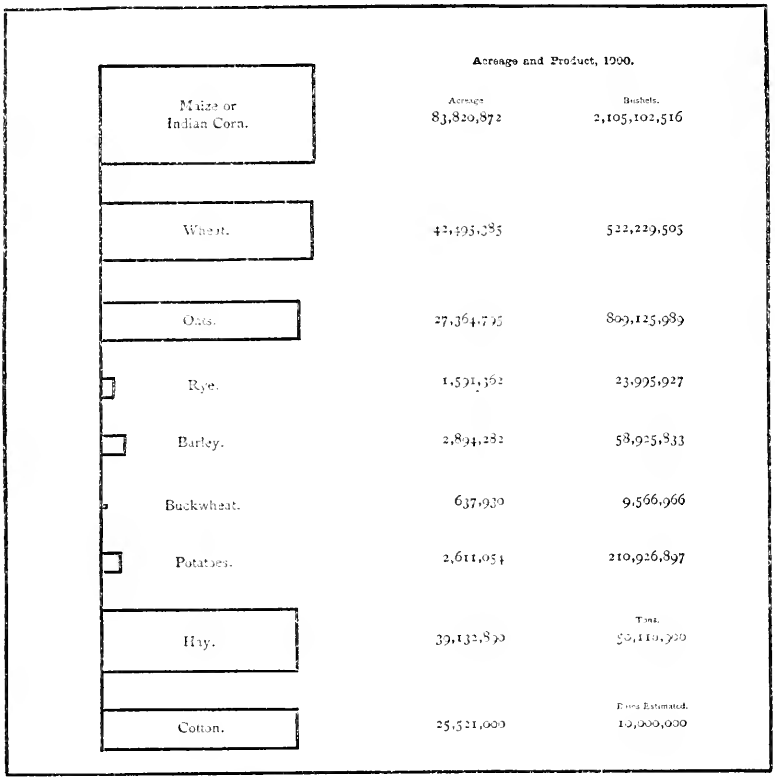
Grain growing States of the Northern Mississippi Valley.

Total Area of Land of the United States Devoted to Maize (Indian Corn), Wheat and Oats. Crop of 1900.

	Square Miles. All States.	12 Mississippi Valley States.	All other States.	Crop Bushels.
Maize.............	125,500	77,794	47,706	2,105,102,516
Wheat	66,400	40,984	25,416	522,229,505
Oats	42,754	30,478	12,276	809,125,989
Total...........	234,654	149,256	85,398	3,436,458,010

Crop of 1900.

	Acreage.	Product.	Average per Acre.
Ohio	1,420,646	8,523,876	The crop of 1900 was below the average per acre of the last ten years. The crop of 1898 was 675,148,705 bushels at 15.3 bushels per acre. The crop of 1901 is officially estimated at 704,000,000 bushels and will probably be more. In 1900 the product of the United States, 522,229,505, was substantially 20 per cent. of world's product of 2,586,025,000 bushels. The land devoted to wheat was 2.3 per cent. of the total area, omitting Alaska. 12.29 bushels per acre.
Indiana....................	1,209,755	6,411,702	
Illinois.....................	1,383,230	17,982,068	
Michigan	1,219,969	9,271,764	
Wisconsin	849,458	13,166,599	
Minnesota..................	4,905,643	51,509,252	
Iowa........	1,397,322	21,798,223	
Nebraska..................	2,066,825	24,801,900	
Missouri.	1,507,737	18,846,713	
Kansas......................	4,660,376	82,488,655	
North Dakota............	2,689,023	13,176,213	
South Dakota............	2,920,244	20,149,684	
	26,230,234	288,126,649	
All other States	16,265,151	234,102,856	
	42,495,385	522,229.505	



The area of the United States (omitting Alaska), a little less than 3,000,000 square miles, is represented by the outer square. The proportion of land cultivated in all the principal crops that count for much in acreage is indicated by the smaller areas computed on the same scale. The total area in the above crops in the year 1900 numbered 353,275 square miles, or about twelve per cent. of the total area.

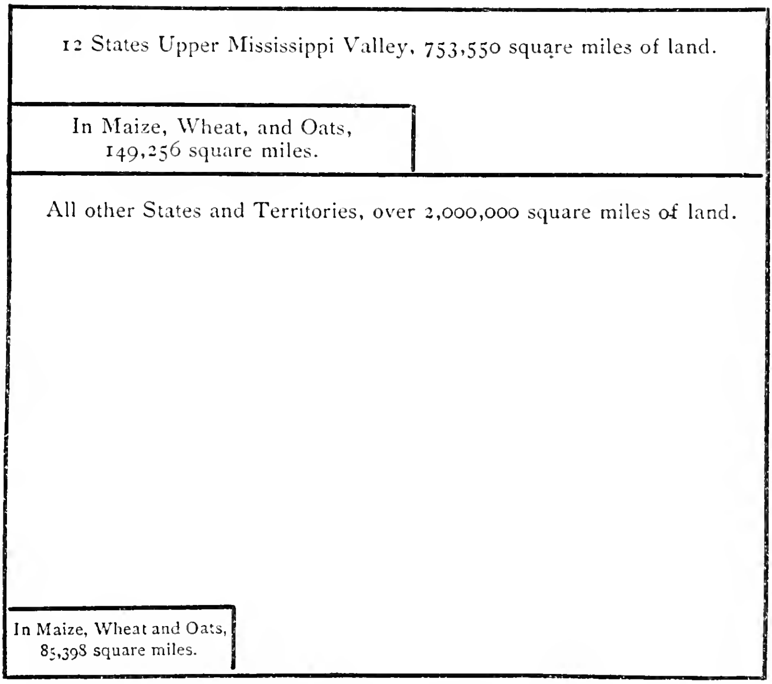
JULY, 1900. COMPUTED BY EDWARD ATKINSON, BOSTON, MASS.

The proportion of land in the twelve Mississippi Valley States listed which is devoted to wheat is five and a half per cent. of their land area.

The change which has come over this section can be put before the imagination in no better way than for the writer to state that he himself witnessed and clearly remembers the war dance of the Blackhawk Indians on Boston Common, when the chiefs were sent around the country by the Government after they had been subdued in the last great Indian war, which occurred in central Illinois, at the very heart of the section with which we are dealing, and very near the present center of population of the country.

The question arises: What is the basis or fundamental principle underlying the change from occupation by the wild or 'blanket'

Indians, as they are called in our country, to the present system of intensive agriculture in a period of less than two generations? I venture to indicate freedom of restriction in custom or law in the purchase and sale and mortgaging of lands as the basis of this great change. The underlying principle, now being rapidly developed, is the gradual change from working land as a mine, subject to exhaustion, and using it as a tool or instrument of production, responding in its product to



Total area of the United States, omitting Alaska, a fraction under 3,000,000 square miles. Area under cultivation in maize, wheat and oats in 1900, 234,654 square miles, or a fraction under eight per cent. of the area of land and inland waters. The latter should be included in the food-producing area.

the measure of mental energy and mechanical aptitude applied to its use. These factors are generated in the free common schools now being supplemented by the addition of manual training schools in all the principal towns and cities.

Dealing in a broad and general way, all are aware that the titles to the vaster portion of the land in these States are derived from the government, mainly since the lay-out of land in sections of six hundred and forty (640) acres each, and quarter sections of one hundred and sixty (160) acres. This land has been disposed of in various ways—to railroad companies as bounties to aid in the construction of railways, and to individual purchasers and settlers under various laws. The

lands granted to private individuals have been, since 1860, usually in small tracts, varying from eighty (80) to one thousand (1,000) acres, the greater share being in tracts known as quarter sections of one hundred and sixty (160) acres. This is the one always required under the Homestead and preemption laws. The lands of railroads were originally granted to them in large tracts. Some of it has been disposed of by them in large tracts to wealthy corporations and individuals and by them converted to vast farms, known usually as 'bonanza' farms. The balance has been sold or is in the process of sale, in smaller tracts on long-time credit to poor settlers. The usual method of sale is one tenth cash and the balance in nine annual payments with interest. The ordinary sale of this railroad land is a tract of one hundred and sixty (160) acres, the one on which the poor settler, as a rule, begins his career on the frontier.

The first settlement of the frontier, as now described, is accompanied with many transfers of land title, owing to the facilities of such transfer and by reason of the benefits that all parties can find in the same. Many who have acquired land under the Homestead law sell it to their neighbors or to a newcomer with ready money. They then take up a new farm under the preemption law, and use the money received for the sale of the first farm to improve the second. The divisions of the bonanza farm, the land of which is usually purchased from the railroads, combined with the results of those transferred among the original settlers, give to the farmer on the frontier an average size of from two hundred and forty (240) to three hundred and twenty (320) acres, the largest containing thousands of acres and the smallest from twenty (20) to forty (40) acres.

When first settled, these large and small farms in the grain-growing sections are always cultivated on an extensive system, largely in grain. The original occupants, whether of large or small farms, turned over the sod and planted their grain with the application of very hard labor and without fertilizers and, as a rule, without any very general comprehension of the art of agriculture. This was the necessity of the case, and it has been and is still a success under the conditions of the frontier. Under these conditions a vast body of pioneers have become prosperous, some of them attaining large wealth, and with their wealth buying out their less successful neighbors and adding to the area of the great farms. That phase has nearly passed by in the great Mississippi Valley and in the States named. With the passage of years, in every part of these twelve States, this extensive system of cultivation comes to an end and the intensive system of working the land in various crops takes its place. With this change the large bonanza farms are broken up and the smaller farm becomes the rule. This change is a progressive one,

as can be seen by the average size of farms in the twelve States. In 1890, this was 86 acres in Michigan, 93 in Ohio, 103 in Indiana, 115 in Wisconsin, 127 in Illinois, 129 in Missouri, 151 in Iowa, 160 in Minnesota, 181 in Kansas, 190 in Nebraska, 227 in South Dakota and 277 in North Dakota. Ohio and Michigan have been the longest settled, while the two Dakotas are not yet wholly occupied by farmers.

On the frontier, the wild land from the government has a value of $1.25 per acre. The railroad grant lands have been usually purchased by farmers at from $4 to $7 per acre and are now being sold at these figures. The average value of farms with improvements in the old settled States, such as Ohio, is not far from $50 per acre. The difference measures the improvements made to the land. To open up new land and make these improvements requires capital. The original settlers, being without capital, were under the necessity of securing credit, either from the railroad companies from whom they purchased the land or from money lenders; hence there grew up in these western States a very extensive system of borrowing on mortgages, beneficial both to borrower and lender, until the speculative mortgage companies promoted the taking up of great areas of land in the semi-arid regions, negotiating mortgages thereon, and thus brought disaster to many lenders, culminating in bankruptcy of many mortgage companies.

The average term of mortgages made for land improvements, as above outlined, has been in the past from three to nine years, and that period of time has usually, except in the semi-arid lands, sufficed to enable the settlers to pay off their debts and to acquire valuable farms from their neighbors.

These figures are sustained by the judgment of the experts now occupied in compiling the census of the year 1900, in which the departments of agriculture and of wealth, debt and taxation are under the supervision of the most competent man in the United States, Mr. L. G. Powers.

It will be plain to any Englishman that unless this land had been free land, bought, sold and conveyed with the least amount of expense and difficulty, and free of any conditions as to the kind of crop to be planted or the disposal of the product, no such great economic revolution could have occurred. Yet the conveyance of land is now being made more simple than ever before by the adoption, in State after State, of a reform which we owe to our intelligent and progressive kindred in Australasia, the registry of titles known as the Torrens System, in place of the registry of deeds. Under this system conveyance of a title to land under absolutely safe conditions has become as simple and as easy as an assignment of a note of hand or a share of stock.

Under these conditions in these and other States 5,700,000 farmers are now working land either as owners or tenants, the only limit in recent years to a further expansion of crops having been the lack of farm laborers. At the present time (July 9th) farm laborers are in most urgent demand to harvest the great crop of wheat, without any sufficient supply.

During the last decade there has been in these States a small lessening in the average area of the farm, coupled with a moderate increase in the number of tenants. The owners have increased faster than the agricultural population, and the greater increase in the number of tenants has been recruited from former farm laborers or from emigrants. Landlordism in the sense of ownership of very large areas to be worked permanently by tenants, covers a very small part of this whole area. It is inconsistent with the whole spirit of the people and will never assume any great importance.

In the new States, such as Minnesota and the Dakotas, it is still possible to buy cheap land from the railroad, from large timber companies and from the State and general government, and to repeat the old process of a poor man acquiring a good farm free from debt in from three to ten years by the aid of a small mortgage loan or the credit of the land companies. In the older settled communities, with land worth on an average from $40 to $50 per acre and in many cases selling for $100 per acre, the road to farm ownership for the poor man is somewhat different. He must as a laborer have acquired money enough to become a farm tenant and as a tenant have obtained sufficient capital to make a reasonable payment on the purchase price of the farm. This gradual rise of a farm laborer to farm ownership through farm tenancy is being witnessed all over the States to which I have referred. Mortgage assists, as on the frontier, in helping the men with small capital to control farms worth more than their resources over and above their liabilities. These men, rising in the older settled States to farm ownership, through farm tenancy and by the aid of mortgage loans, in a large measure succeed in paying off their debts as does the settler on the frontier. With a large debt due on the more valuable farms, it may require a longer time, but the end is reasonably sure with those of any business sagacity.

Another class of farm tenants in America is composed of the children of the farm owners who cultivate their fathers' lands as tenants until they succeed them as owners. No farm laborer who is not a good farmer succeeds in rising, and the sons of wealthy land owners, without good management, often lose all their inherited wealth. Freedom to buy and sell and manage land kills off the incompetent, and gives the field to the competent, be he poor or rich. This freedom of land sales prevents the tenant or owner

from adopting methods formerly described by Governor Wise of Virginia, as that of slavery, when he said that 'the white men skinned the nigger and the nigger skinned the land.' There is an element of skinning in every system relating to land not born of perfect freedom. Perfect freedom in the purchase and rental of American land leads to constant improvement.

Under the freedom of sale which prevails in the United States, with the facility of mortgage loans which permits the poor man to use the capital of the rich to secure for himself a farm, there will always be a large mortgage debt in the rural sections of the United States. That debt marks, as a rule, the upward movement of the poor laborer on the road of farm ownership. One class of men incur debt for land purchased or for improvements, and pay off the same, and as they retire in old age another and younger set repeat the process of rising to independence by the same road. The relative number of those who have attained their goal and of those on the way may be seen by the following figures:

Of the farms in the twelve States named, about sixty to sixty-five per cent. are now free from mortgage debt, and thirty-five to forty per cent. are mortgaged. The debt on the mortgaged farms does not exceed thirty-five per cent. of their value, and the total mortgage debt of the States is not in excess of about twelve to fifteen per cent. of the whole farm value.

Under these conditions the area of land devoted to the several grain crops diminishes in ratio to that given to other crops, varied farming taking the place of the all-wheat or all-corn system. But by the introduction of intensive farming there is a steady improvement and increase in the quantity and the quality of the crops derived from a given area of soil. The wheat needed for home use will keep even with population for many years without any increase in area.

The most potent agency in this revolution in agriculture may be but little known in Europe, especially in England. I refer to the so-called Agricultural Experiment Stations, which have grown in a rather singular manner, of which no very definite record has yet been given. The general government appropriates annually $720,000, and the State governments $440,000, more or less, in addition, to be expended by the Agricultural Experiment Stations, under the general supervision of a special department in the Department of Agriculture, of which Professor A. C. True is now the director. But the general government has no very definite control over the expenditure of this money. The stations are established by the several States. They are now thirty-six in number—one in nearly every State; two or three in some of them. Each one is under the direction of a trained student of the science of dealing with land as an instrument or tool of produc-

tion. The employees are all thoroughbred experts, graduates of universities, colleges or technical schools. A more devoted set of men cannot be found in the whole country. Their influence is raising the standard of farming in almost every State. One of the great railway promoters in the Northwest long since realized the importance of this matter, and in order to promote the interests of his railroad he made arrangements with every town throughout the great State near the line to send two men every year to the capital, where the Agricultural College and Experiment Station were situated. He gave them a free pass, liberty to remain four days, the State giving them a banquet—the only condition being that one day out of the four should be devoted to the study of the work of the Agricultural Experiment Station; and for a number of years one thousand (1,000) to fifteeen hundred (1,500) men enjoyed this benefit every year.

Each Experiment Station devotes itself to the special conditions of the State in which it is placed. For instance, in Minnesota the whole standard of wheat cultivation and of dairy product has been raised to a very high point. In Michigan the market value of a large product of butter has been raised to a more profitable point by improvement in stock, in the establishment of creameries, and in other ways. The application of the bacterium of June butter in the creameries has become common.

Professor Kohn, who made this discovery at the Columbian Exhibition in Chicago, in 1893, has established a bacterium factory which, at the last advices known to me, supplied one hundred and fifty (150) creameries with the ferment.

In Kansas attention has been given to the improvement in the quality of maize or Indian corn. The ordinary maize is deficient in the nitrogen or protein elements as compared to the starch and fats. Varieties have been bred, bringing maize even with wheat in the protein element, which may end in making maize as complete a food as either wheat or oatmeal.

In the South the greatest attention is given to renovating plants of the leguminous type—beans, peas, alfalfa and others. The slave-stricken lands are being regenerated, and gradually but slowly stock suited to the climate and conditions of the Atlantic cotton States is being introduced.

Of course all these changes imply the use of fertilizers in larger and larger measure. That need is being met. The vast deposits of phosphatic material in Kentucky, Tennessee, Florida and the Carolinas, coupled with the use of the ground slag from basic steel furnaces, give an assurance of an abundant supply of that necessary element for all time to come.

The discovery of the function of the bacteria attached to the stalks

of leguminous plants, dissociating the nitrogen of the atmosphere and converting it through the plant to the renovation of the soil, coupled with artificial sources, give assurance of adequate supply of that necessary element, while discoveries in science promise yet greater abundance in the conversion of the secondary products of gas plants to fertilizers.

The last element which is necessary, potash, of course exists in great abundance throughout many sections of the country, but the solubility of potash capable of being assimilated by plants and the cost of deriving it from its original source in the rocks, have rendered the country for the time being largely dependent on the Stassfurt Mines of Saxony, where the existence of a pan underlying the salt in which the potash has accumulated, has rendered that place the source of this necessary element in fertilizers at the lowest cost. It is, however, hardly to be doubted that in the great range of alkali soils and deserts extending from British Columbia around the circle far into Texas, deposits of potash will soon be discovered which can be worked. Permanent potash springs are very numerous, and in the arid country it may be assumed that while the potash may have leached down to a moderate distance, it has not been carried away. A strong company, with abundant capital, under competent engineers, has lately been organized for following the surface indications of potash by boring at many points.

In a broad and general way it may be safely affirmed that the great farming States of the Mississippi Valley which have been named, will produce this year within a fraction of all the wheat now required for the consumption of the people of the United States, and that by improvement in the methods of agriculture their product will keep even with the increase of population without calling for more land. Outside this area are vast sections from which the quantity of wheat now available for export may be derived. In these sections the intensive system has not yet taken the place of the former methods of cultivation. It may be safely affirmed that Montana, Washington, Oregon, California and other sections of the Northwest and of the Pacific coast, can produce all the wheat that Europe can possibly pay for during the present generation. It is only a question of price. Our crop now being marketed officially estimated at 704,000,000 is probably 750,000,000 bushels or about 95,000,000 quarters. The prevailing drought did not come until the winter wheat was harvested and the spring wheat fairly secure; it will reduce the corn or maize crop. At a dollar a bushel or at thirty-two shillings per quarter in Mark Lane, we could add 20,000,000 quarters in a year or two if we had the farm laborers to do the work not yet done by machinery.

Again, although the cotton States of the Atlantic coast will not be great producers of grain, especially of wheat, yet in the Southwest—in Texas, Oklahoma, the Indian Territory and Louisiana—we could readily produce the entire wheat crop of the United States upon unoccupied land, whenever labor and capital can be found sufficient to develop the product. As our country people say, a dollar a bushel would fetch it. Some of the best hard or macaroni wheat in the world is already produced in this section.

I have called the attention of economists to the basis of this development of agriculture, namely, what may be called the free land tenure established in the United States. An outsider should deal with the conditions of other countries with great caution, but in the study which I have given to the subject it has seemed to me very plain that the feudal land system of the United Kingdom of Great Britain and Ireland had come to its necessary end, witnessed by the present movement for the practical confiscation of land titles in Ireland. One may ask, what would be the potential of the land of the United Kingdom in the production of food for its own population, if the purchase and sale of land were as free as it is in the United States?

Again, it appears to an outsider as if the revulsion from the feudal system in France and large parts of Germany, where the land is cut up in little patches, had also failed in developing the potential of the soil, the application of modern mechanism to its full effect being rendered impossible by the great subdivision of the soil.

You will remark that the area of the United States, omitting Alaska, covers three million (3,000,000) square miles; the habitable part of Canada may be computed at over two million (2,000,000), to which we may add Mexico, giving in all, say over five million (5,000,000) square miles, of which more than one-half is available for cultivation. At nine thousand (9,000) to ten thousand (10,000) bushels to a square mile, which is rather a low standard of intelligent cultivation, ten (10) per cent. of this area, or two hundred and fifty thousand (250,000) square miles, would yield about the present wheat crop of the world. I think we could spare that area without missing it, even within the limits of the United States, if we could make a contract for a term of years at thirty-two (32) shillings a quarter in London for all the wheat Europe could possibly buy.

Even if this forecast be considered visionary, it may be surely held that the United States can supply for many years to come the entire deficiency in the wheat crop of the United Kingdom, twenty-five million (25,000,000) to thirty million (30,000,000) quarters a year, probably by improvement in intensive farming, without adding materially to the land which may be devoted to the wheat crop.

In this paper I have given the details of the grain problem. Cotton

comes next in importance, especially to the people of Great Britain. The all-cotton, old plantation system is extinct. A mere fraction of the present cotton crop is growing in the old way; almost the whole comes from the small farmers, black as well as white. The tenant system was almost universally adopted in the process of reconstruction; improvement in agriculture is slow but sure. The dream of the freedman was forty acres and a mule, and in fact great numbers are attaining that end.

For many years after the end of the Civil War the cotton States still depended upon the North for hay and upon the West for corn and meat. There is probably no great force of laborers in the world who can fully subsist at so low a cost as the Southern negroes. 'Hog and hominy,' as it is called, bacon and cracked corn, are their choice above all other kinds of food. On this ration, coupled with such fruits and vegetables as they can secure, they are content. A peck of corn meal, three and one-half pounds of bacon and a quart of molasses or sorghum syrup is the customary ration for one week, costing six to nine cents a day. All that is changing. The intelligent farmers now produce their own bread and meat; some of them in excess. They are developing leguminous plants—pea vines, beans, alfalfa, crimson clover and the like; gradually introducing stock, and soon to fold sheep upon the cotton fields, to the renovation of the soil.

The very large proportionate number of tenants which has been disclosed by the former census and will be yet more marked in the present census is mainly the result of the changing conditions in the cotton States; a passing phase in the South, as it is in the West; not of long duration, and not implying any permanent condition of landlordism.

In fact, in conclusion it may be dogmatically stated that both wheat and cotton are becoming the excess, surplus or money crops of farmers whose products otherwise suffice to sustain the farm. It is therefore difficult to measure the exact cost of raising wheat. It has been produced at less than one shilling per bushel, including use and repairs of machinery and interest thereon, but not including any charge for the rental of land, which forms a part of the income or profit of the farmer. It may be dogmatically affirmed that so long as the farmer in the Mississippi grain-growing States can secure to his own use and enjoyment one cent a pound, sixty cents a bushel, four dollars and eighty cents, or twenty shillings a quarter, the present average product of wheat will be maintained, subject to variation in quantity according to the season. At seventy cents a bushel new land will be put under cultivation in wheat to any extent of the demand, and capital will be found. The difficulty will be to procure even the necessary labor still required, notwithstanding the increasing use of machinery and

the common practice of small farmers in combining for the ownership of mowers and reapers or in contracting for the harvest.

No subject of greater importance could be brought before the English-speaking people; none of greater weight in maintaining our interdependence with our kin beyond the seas. The right comprehension of this problem will give assurance of peace, good-will and plenty.

It may be interesting to call your attention to the fact that the Pilgrim Fathers spent several years in Holland before they migrated to New England. The larger part both of Pilgrims and Puritans came from the southeastern counties of England, where institutions had been greatly modified by Dutch, Flemish and Huguenot immigrants. The Dutch themselves settled New York and other colonies. We derive our common schools, our toleration of religion, our welcome to invention and our free division of land chiefly from the Dutch, rather than from our English ancestors. It is true that the Puritans were intolerant and that the Dutch attempted to establish large manors in the State of New York, under patroons, so-called; but the more liberal tendencies of the Pilgrims in New England, the Quakers in Pennsylvania, the Baptists of Rhode Island, the Dutch in New York and the Catholics in Maryland overcame the intolerance of the Puritans, while the free system of land holding also displaced all other tenures.

THE INERT CONSTITUENTS OF THE ATMOSPHERE.

By Professor W. RAMSAY,

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THE discovery of an element always awakens interest; for the total number of the known elements does not exceed seventy-five, and all the various forms of matter which exist on this globe are necessarily composed of these elements. An element, as is well known, is the ultimate constituent of a compound; and with only a limited number, Nature has provided us with that enormous wealth of minerals, of vegetables and of animals, all of which have as their constituents two or more of these elements.

These elements, however, must not be regarded as isolated entities, each self-dependent, having no relations with its compeers; on the contrary, all the elements exhibit certain connections with their neighbors; and there is to be traced an orderly progression from one class of elements, strongly electro-positive in character, metallic in appearance, very inflammable when heated in the air, and at once attacked by water, to another class, highly electro-negative, transparent, unattackable by oxygen, and without perceptible action on water, through a number of connecting links, each of which serves to soften the transition.

These elements have been arranged in series, and it is by considering the method of arrangement that our interest is awakened. The earliest attempt to make such an arrangement antedates the very idea of the conception of an element. For the division of all matter into metal and non-metal is one which is lost in the mists of antiquity. The word 'metal' is derived from the Greek verb μεταλλάω, I search; and that verb is said to be derived from μέτα and ἄλλα, signifying 'after other things.' As it was recognized that elements are constituents of more complex matter, a conception first emphasized by Boyle, and as the distinction became clear that matter which resists decomposition must be classed as elementary and, after a century and a half, a number of elements were recognized, it was obvious that a number of them might be grouped in classes. Take, for example, the elements chlorine, bromine and iodine, all colored, strongly smelling substances, sparingly soluble in water and forming compounds barely distinguishable from each other in appearance or by a cursory inspection; or take such a group as the metals of the alkalies, lithium,

sodium, potassium, rubidium and cæsium, all white, soft metals, all easily oxidizable, all at once violently attacked by water, and generally with such energy as to be inflamed at the contact. It required no great penetration to class such elements as these into classes.

The revival of the hypothesis of the atomic constitution of matter by Dalton and of his attempt to determine the atomic weights of the elements was not long in provoking the guess that perhaps there could be found some connection between the numbers representing the relative atomic weights of kindred elements. But, as is well known, the state of knowledge in Dalton's day was not sufficiently advanced to enable him to attribute to elements their correct relative atomic weights; and it was not until the eminent professor of chemistry in Rome, Cannizzaro, whose jubilee has recently been celebrated, pointed out the bearing on Dalton's numbers of all the facts accumulated up to the year 1856 that the close relationship between the atomic weights and the properties of the elements was suggested by John Newlands. Some years later, Lothar Meyer and Dmitri Mendeléef amplified and elaborated the ideas which had first been propounded by Newlands; and the periodicity of the atomic weights and the gradual variation of the properties of the elements and their compounds were established on a firm basis.

Various plans have been adopted to render this arrangement pictorially visible; each method has perhaps its own conveniences, but none can be regarded as the method *par excellence.* Lothar Meyer's original table is constructed on the hypothesis that a cylinder, on which the numbers have been distributed in their order on a descending spiral in eight main columns, has been unrolled.

Another method of representation is due to Dr. Johnstone Stoney. The atomic weights are represented on a spiral curve, closely approximating in form to a logarithmic spiral, and the magnitudes of the atomic weights are represented by the volumes of concentric spheres. Thus, the sphere in the middle stands for unity, the atomic weight of hydrogen. The elements follow each other according to the numerical order of their atomic weights; and by joining the points thus obtained, a nearly regular spiral curve is produced, resembling one derived by aid of a logarithmic or elliptical formula. The deviations from regularity appear also to follow a law, and if accurately mapped the spiral is a sinuous one. But the determination of individual atomic weights is as yet not sufficiently accurate to make it possible to calculate the course of the wavy line.

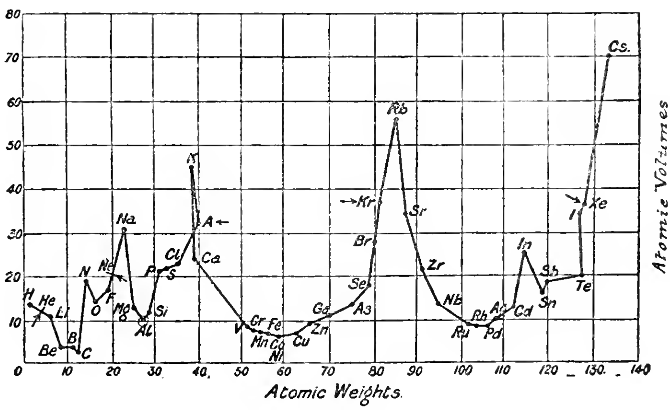
A third diagram, modified from that of Meyer, has been constructed by Professor Orme Masson, of Melbourne. The chief difference is that instead of grouping elements of the iron, palladium and platinum groups, they are distributed; hydrogen forms the first element of the

fluorine column; and the long and short periods are kept separate by a fold in the diagram. A diagonal line, too, divides the 'metallic' from the 'non-metallic' elements.

Other devices have been suggested in order to represent diagrammatically the relations between the atomic weights; but it must be borne in mind that whatever system is employed, such plans are merely aids to thought, and have no real significance. They are on a par with the representation of numerical relations as curves, and can convey nothing which is not already contained in the actual numbers.

It was Mendeléef who first drew attention to the progressive alteration of the valency of the elements in passing from left to right along the table. While the metals of the alkalies, lithium, sodium, potassium, rubidium and cæsium, are all monads, in as much as one atom of any one of these elements is able to replace one atom of hydrogen, the typical monad, elements of the beryllium group are dyads; hence while the formula of sodium chloride is NaCl, that of calcium chloride is $CaCl_2$, that of boron chloride, BCl_3, for boron is a triad; the chloride of the tetrad, carbon, CCl_4, and so on. And considering the compounds with hydrogen, where these exist, we have BH_3, corresponding to the chloride; CH_4, NH_3, OH_2, and finally ClH and FH. As the valency alters by unity in each case, it appeared reasonable to place the elements on the table in equidistant columns; or, as in Dr. Stoney's diagram, on equidistant lines, dividing the spiral curve into eight equal segments.

Meyer, however, showed that if the elements be mapped on square



paper, so that the vertical divisions correspond to the volumes occupied by unit weight of the solid or liquid elements, while the horizontal divisions correspond with the atomic weights, a certain amount of

regularity is to be noticed, as is to be seen in the accompanying diagram. There, it will be noticed, the elements, sodium, potassium, rubidium and cæsium, occur at the summits of somewhat irregular curves. Such relations, among others, led him to formulate the proposition that the properties of the elements are periodic functions of their atomic weights; that is, they vary in a systematic manner, either positively or negatively, as the scale of the atomic weights is ascended.

The division of the elements into metals and non-metals corresponds broadly with another well-marked division—that into basic and acidic. Generally speaking, it is the oxides of the metallic elements which react with water to form bases; and those of the non-metals which form acids with water. This distinction was recognized by Lavoisier, when he named oxygen the acid-forming element. But what is a base? And what is an acid? The old definition was—two classes of substances, which, when brought together, react to form a salt. But the definition may now be made with greater definiteness. It was known to Cavendish and to Priestley, that when a current of electricity was passed through the solution of a salt in water, one portion of the salt, namely the basic portion, came towards the negative pole; and it was believed that this was due to the basic portion possessing a positive charge. Similarly, the acid portion, possessing a negative charge, traveled towards the positive pole. Hence, bases were said to be electro-negative, and acids, electro-positive. And Sir Humphry Davy arranged the elements in a series, of which one was supposed to be electro-positive to its neighbor on the right, and electro-negative to its left-hand neighbor.

According to modern ideas, bases, by the mere act of solution in water, are supposed to be split up into two portions, for which the term ion, invented by Faraday, has been retained; one ion is charged by the process of solution with a positive charge, and that portion is usually a metal; the other portion, which consists of one or more groups of hydrogen and oxygen in combination, termed 'hydroxyl'—OH—has a negative charge. A base, indeed, is a compound which splits in this manner. On the other hand, an acid, when dissolved in water, undergoes an analogous split; but in this case the electro-positive ion is always hydrogen, while the electro-negative ion may either be an element such as chlorine, or a group of elements such as exist in nitric acid (NO_3).

The order of the various elements in the electric series has been determined; and not merely determined, but to each has been attached a numerical value. This value is identical with what is termed 'chemical affinity'; and it represents the electric potential of the element with reference to an arbitrary starting-point, which does not differ much from that of nickel, an element closely related to iron. Only

a few such values have as yet been determined numerically; instances may be chosen from the magnesium group, where the numbers run: Magnesium = + 1.2; Zinc = + 0.5; Cadmium = + 0.19; or from the fluorine column, where the numbers are: Fluorine = — 2.0; Chlorine = — 1.6; Iodine = — 0.4. In each case the potential, positive or negative, is the highest for the element with smallest atomic weight, and decreases with increase of atomic weight, for elements in the same column. The order of some of the elements is: Cs Rb K Na Li Ba Sr Ca Mg Al Mn Zn Cd Fe″ Co Ni Pb H Cu Ag Hg′Pt″″ Au‴; and for electro-negative ions, S″ O″ I Br Cl F; the first element, cæsium, being the most electro-positive, and the last, fluorine, the most electro-negative.

The order given above corresponds fairly well with the order in the periodic table, passing from left to right. But, as in the table, the atomic weights follow each other continuously round the cylinder or round the spiral, the abrupt change from elements of an extreme electro-negative character, like fluorine to sodium, an element of highly electro-positive character, or from chlorine to potassium, has always appeared remarkable. The old dictum, *Natura nihil fit per saltum,* if not always true (else we should have no elements at all, but a gradual and continuous transition from one kind of matter to another—a condition of affairs hardly possible to realize), has generally some spice of truth in it; and it might have been predicted (and the forecast seems to have been made obscurely by several speculators) that a series of elements should exist which should exhibit no electric polarity whatever. Such elements, too, should form no compounds, and, of course, should display no valency; they should be indifferent, inactive bodies, with no chemical properties.

The discovery of argon in 1894, followed by that of terrestrial helium in 1895, and of neon, krypton and xenon in 1898, has shown the justice of the foregoing remarks. In as much as the methods employed for the isolation of these elements illustrate their properties and confirm the views as to their inertness and lack of electric polarity, I propose to sketch shortly the history of their discovery.

An accurate investigation of the density of atmospheric nitrogen and of nitrogen prepared from its compounds led Lord Rayleigh to inquire into the cause of the discrepancy, for the density of the nitrogen of the atmosphere was found to exceed that of 'chemical nitrogen' by about one part in two hundred, whereas the accuracy of his experiments was such that it would have excluded an error of one part in five thousand. I need not here allude to the reasons which were at first put forward to account for this anomaly; suffice it to say that they offered no explanation; and that we ultimately traced the discrepancy to the presence in 'atmospheric nitrogen' of a gas nearly half as dense again

as nitrogen. Two methods were adopted for isolating this gas. One was a repetition of a process which had been employed by Cavendish in 1785; it consisted in passing electric sparks for a long time through air, confined over mercury, in presence of caustic alkali. The accompanying woodcut gives an idea of the apparatus he employed. This experiment had been made by Priestley ten years previously, but not with quantitative accuracy. It was Cavendish's object to inquire whether the nitrogen of the atmosphere had any claim to be regarded as a homogeneous substance, but he left the question undecided. Having continued to pass sparks through a measured volume of air, with the occasional addition of oxygen, until no further diminution in volume occurred, he found that after the excess of oxygen had been removed, the residue amounted to not more than 1/120th part of the whole of the nitrogen. The actual volume of the inactive gases in the nitrogen of the atmosphere is one eighty-fourth. Cavendish did not pursue the investigation further, and the discovery of argon was postponed for a century.

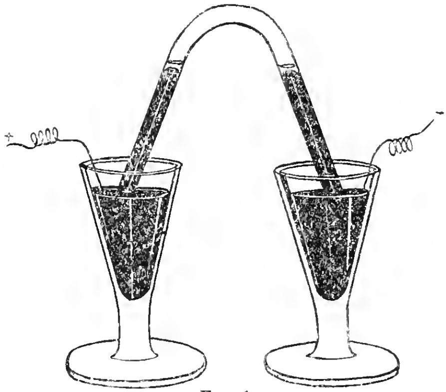


Fig. 1.

A convenient form of apparatus for repeating Cavendish's experiment is shown in the accompanying figure. The gas, air mixed with oxygen, is confined over mercury in an inverted test-tube, in contact with a few drops of a solution of caustic potash; and by connecting the rings with wires from the secondary coil of an induction apparatus, sparks pass between the platinum terminals in the interior of the test-tube. The volume of the gas rapidly diminishes; and in a few hours, the gas is removed to a clean tube, and the excess of oxygen absorbed by burning phosphorus; the inert gases remain behind.

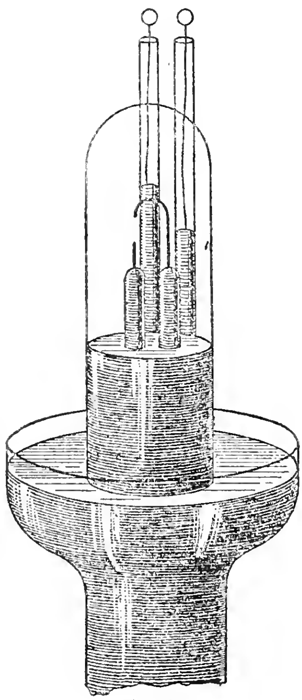


Fig. 2.

On a larger scale, the apparatus used by Lord Rayleigh, consisting

of a balloon of six liters capacity, in the interior of which an electric flame is kept alight by means of a transformer, while a jet of caustic alkali forms a fountain in the interior, gives good results. By its help, seven or eight liters of mixed gases can be made to combine per hour.

Such experiments show the inactive nature of the argon group of gases towards an electro-negative element, oxygen. The gases are absolutely incombustible. No other elements can withstand such treatment, save platinum and its congeners, and gold. But even these metals combine with fluorine or chlorine, when heated in a current of one or other gas. Argon, however, is wholly unaffected when electric sparks are passed through its mixture with chlorine or fluorine, the two other most electro-negative elements. To them, too, it shows itself completely indifferent.

A more convenient method of separating the nitrogen from its admixture with argon in atmospheric air is by means of red-hot magnesium. The metal magnesium, which is now made on a considerable scale for photographic and signaling purposes, is a white, silvery metal, which can be planed or turned into shavings. In the early experiments, a measured quantity of atmospheric nitrogen, dried by passing over suitable drying agents, was brought into contact with magnesium turnings, heated to redness in a tube of hard glass. It has been found, however, by M. Maquenne, that the metal calcium, which, for this purpose is most easily produced by heating together a mixture of magnesium filings and pure dry lime, is a more efficient absorbing agent for nitrogen, for it does not require such a high temperature, and can be effected without danger of melting the glass tube. Indeed, the operation is a very easy one, and can be carried out with the very simple apparatus shown in Figure 3. M. Guntz has also found that lithium, an element belonging to the same column in the periodic table as sodium and potassium, is an exceedingly good absorbent for nitrogen, for it tarnishes in nitrogen even at atmospheric temperature, owing to the formation of a nitride.

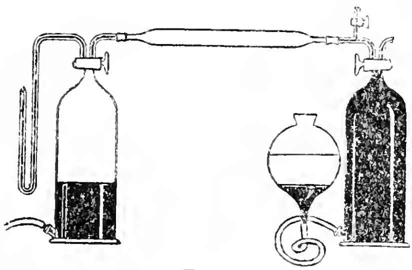


FIG. 3.

On a large scale, the magnesium turnings are contained in iron tubes, and the gas-holders are made of copper or of galvanized iron. By this means, fifteen liters of argon were separated from about two cubic yards of air.

The inactivity of argon in contact with such highly electro-positive elements as lithium, magnesium and calcium again demonstrates its

want of electric polarity. No other elements would have resisted such treatment, except those of the argon group. But these are not the only data from which such a conclusion can be drawn; for it was found that no action takes place between argon and hydrogen, phosphorus, sulphur, tellurium, caustic soda, potassium nitrate, sodium peroxide, sodium persulphide, nitro-hydrochloric acid, bromine-water and many other reagents which it would be tedious to mention, all of which are remarkable for their chemical activity. We may therefore take it that the name 'argon,' which means 'inactive,' has been happily chosen.

In attempting to form compounds of argon, however, another consideration was not lost sight of; if compounds of argon were capable of existence, they ought to exist in nature; and as in all probability they would be easily decomposed by heat, it ought to be possible to decompose them with evolution of argon, which could be collected and tested. Professor Miers, in a letter which he wrote me the day after an account of the fruitless attempts to cause argon to combine had been given to the Royal Society, drew my attention to experiments by Dr. Hillebrand of the United States Geological Survey, in course of which he obtained a gas, which he believed to be nitrogen, by treating the rare mineral clevite, a substance found in felspathic rocks in the south of Norway, with sulphuric acid. The chief constituents of clevite are oxides of the rare elements uranium and thorium, and of lead. The gas obtained thus, after purification from nitrogen, was examined in a Plücker tube with the spectroscope, and exhibited a number of brilliant lines, of which the most remarkable was one in the yellow part of the spectrum, similar in color to the light given out by the glowing tube. The position of this line and of others which accompany it established the identity of this gas, not with argon, as was hoped, but with a supposed constituent of the sun's chromosphere, first observed by M. Janssen of Paris, during an eclipse which was visible in India in 1868. The late Sir Edward Frankland, and Sir Norman Lockyer, who studied the spectrum of the chromosphere, gave to the supposititious element, which they regarded as the cause of these lines, the name 'helium,' a word derived from 'ἥλιος,' Greek for 'the sun.' Having been placed on the track, I examined, with the assistance of Dr. Collie and Dr. Travers, no fewer than 51 minerals; while Sir Norman Lockyer examined 46 additional ones, which we had not examined; and in 19 minerals, almost all of them containing uranium, helium was found. Only one gave an argon spectrum, namely malacon. We also sought for argon and helium in meteorites, which all give off gas on heating; but in only one specimen, a meteorite from Augusta County, Virginia, was helium found, in this case accompanied by argon. All natural waters contain argon, for

that gas is somewhat soluble in water (4.1 volumes per 100 of water at 15° C.); but some also contain helium, as for instance the gas from the Bath springs, which Lord Rayleigh found to contain argon mixed with about 8 per cent. of its volume of helium; and helium has also been found in mineral springs at Wildbad, and at Cauterets, in the Pyrenees. It would appear, then, that helium is not such a very rare constituent of our globe; and indeed, it is probable that it is continually escaping from the earth in small quantities in certain regions.

Let us next turn our attention to the atomic weights of these elements, in order to discover what position should be assigned to them in the periodic table. It is not difficult to ascertain their molecular weights; that is the relative weights of equal numbers of molecules; for, assuming Avogadro's hypothesis, that equal volumes of gases contain equal numbers of molecules, or particles capable of independent existence in space, the weights of equal volumes of these gases, compared with that of an equal volume of oxygen taken as 16 (the usual standard) will give the relative weights of their molecules. The density of helium was found to be very nearly 2, or one eighth of that of oxygen; while that of argon was 19.94, very nearly 20. It may be interesting to spend a few minutes in a description of the method by which the density is determined. The principle is to weigh a globe, completely emptied of air by means of an air-pump; the globe is then filled with the gas, care being taken to observe accurately the temperature and pressure of the atmosphere at the moment of closing the globe; and the difference in weight of the full and the empty bulb gives the weight of a known volume of the gas. It is easy to compare it with that of an equal volume of oxygen.

The weight of a gas is much more considerable than might be supposed. Thus, a liter of oxygen weighs nearly a gram and a half; and the air in an ordinary room twelve feet broad, long and high, weighs over a hundred and fifty pounds. It is possible to obtain fair results by weighing as little as 30 cubic centimeters, or about one fluid ounce of any gas. Such a globe filled with helium weighs about one two-hundredths of a gram; and it is not difficult to be fairly certain to one-hundredth part of that weight. With a heavier gas like argon, much greater accuracy is of course possible.

Now, although the standard atomic weight with which others are compared, that of oxygen, is taken as 16, it is believed, for reasons which will afterwards appear, that a molecule of oxygen consists of two atoms, the weight of which will of course be 32. And, as the weight of helium is one eighth of that of an equal volume of oxygen, the weight of a molecule of helium will be the eighth part of 32, or 4. Similarly, the molecular weight of argon compared with that of an atom of oxygen taken as 16 will be 40. But the question has still to

be answered: Does a molecule of helium or of argon resemble a molecule of oxygen in consisting of two atoms; or does it consist of one atom or of more than two?

It is believed that when heat is put into a gas, it is expended in causing the molecules to move. This motion may in some cases be of two kinds; the molecules may be urged through space, each molecule traveling in a straight path, until a collision takes place with another molecule, when it changes its rate and direction of motion; such motion is termed 'translational motion.' On the other hand, if the molecules are themselves complex, that is, if they consist of groups of atoms, any energy imparted to the gas in the form of heat will produce, not merely the translational motion, but will also cause the atoms to move relatively to each other within the molecule. It is only the translational motion which is manifested as pressure, for it is only by their motion through space that the molecules can bombard the sides of the vessel which contains them, and so exert pressure on the walls. Hence it will require a less amount of heat to raise pressure in a gas with simple molecules, than in one of which the molecules are complex, for in the former case no heat is used in causing internal motion. Now, to measure such quantities of heat is by no means easy, although it has been successfully accomplished in some instances. To avoid this measurement, a device is adopted which produces equally satisfactory results. It consists in comparing the amounts of heat required to raise the temperature of a gas, first, when it is not allowed to expand, and when all the heat is used in producing molecular motion of the kind referred to; and second, when it is allowed to expand, and consequently when it could be made to do work; for example, to drive a small air-engine. In the latter case, a greater amount of heat is required to rise the temperature of the gas; an amount equivalent to the work which the gas does on expanding. This quantity, however, which is equivalent to mechanical work, is the same for all gases, provided equal numbers of molecules (or equal volumes) be heated through the same number of degrees of temperature. And this renders it possible to calculate the amount of heat required to raise the temperature of a gas, even without a direct measurement. An example will serve to render this somewhat difficult conception clear. For mercury-gas, for argon and for helium, and indeed for all gases, nitrogen, oxygen and their mixture, air, if a volume which contains the molecular weight of the gas taken in grams be raised through one degree of temperature, allowing the gas to expand, and so to do work, the amount of heat equivalent to this work is sufficient to raise the temperature of two grams of water through one degree. This is termed in the language of heat-measurement 2 calories. Now, the total heat required to raise the temperature of 40 grams of argon, for example (and it must be remembered that 40 is

the molecular weight of argon), through 1°, allowing it to expand while it is being heated, is 5 calories. Deducting the 2 calories required for external work, 3 calories remain as the heat required to raise the temperature of the gas. For oxygen, on the other hand, the heat required to raise the temperature of 32 grams, its molecular weight, through 1° is 7 calories; and deducting 2 as before, the remainder is 5, the specific heat of oxygen. Hence for argon and for oxygen, we have the properties:

	Heat Required.						
	No external work.		External work.				
Argon	3	:	5	::	1	:	$1\frac{2}{3}$
Oxygen	5	:	7	::	1	:	$1\frac{2}{5}$

The argument stands thus: The heat required to raise the temperature of argon without expansion can be accounted for entirely on the supposition that it is wholly used in causing the molecules to move through space; on the other hand, more heat requires to be communicated to oxygen than to argon in the proportion of 3 to 5. With oxygen and similar gases, this extra heat must be doing something; it is supposed to produce motion of the atoms within the molecule. There is no such motion within the argon molecule; hence it is concluded that the molecule consists of a single atom; and in that case, the molecular weight is the same as the atomic weight. The molecule of oxygen may be considered as possessing a structure like that of a dumb-bell; the atoms forming the knobs at each end of the bar. On throwing a dumb-bell through space, it will not merely change its position as a whole; but it will rotate. But a molecule of argon or helium is imagined to have the simpler form of a sphere or ball; when it is thrown practically no energy is used in causing it to rotate, but it is all expended in making it pass through space.

I must apologize for introducing such abstruse conceptions into a popular exposition; but they are necessary to the argument; and I am afraid that no simpler means can be found of reaching the conclusion that the molecules of argon and of helium are identical with their atoms.

As 4 is the molecular weight of helium, and as 40 is that of argon, these numbers also stand for their atomic weights. Let us next see how these figures fit into the periodic table.

In 1897, as president of the Chemical Section of the British Association, I chose the title 'An Undiscovered Gas' for the address to the Section. The arguments in favor of the existence of such a gas were briefly these: The differences between the atomic weights of consecutive elements in the columns of the periodic table are approximately 16 to 20; thus 16.5 is the difference between the atomic weights of fluorine and chlorine; 16, between those of oxygen and sulphur, and so on.

Again, stepping one pace down the scale, we have 19.5 as the difference between chlorine and manganese; 20.3, between sulphur and chromium; 19.8, between silicon and titanium, etc. The total difference between manganese and fluorine is 36; between chromium and oxygen, 36.3; between vanadium and nitrogen, 37.4, and between titanium and carbon, 36.1. This is approximately the difference between the atomic weights of helium and argon, 36. I quote now from that address: "There should, therefore, be an undiscovered element between helium and argon, with an atomic weight 16 units higher than that of helium, and 20 units lower than that of argon, namely 20. And if this unknown element, like helium and argon, should prove to consist of monatomic molecules, then its density should be half its atomic weight, 10. And pushing the analogy still further, it is to be expected that this element should be as indifferent to union with other elements as the two allied elements."

Those who care to read the story of the search for this undiscovered element may find it in the address. Minerals from all parts of the globe, mineral waters from Britain, France and Iceland, meteorites from interstellar space; all these were investigated without result. Helium from various minerals was separated by long and tedious processes of diffusion into a possibly lighter portion, diffusing more rapidly, and a possibly heavier portion, diffusing more slowly; but with no positive result. The systematic diffusion of argon, however, gave a faint indication of where to seek for the missing element, for the density of the more rapidly diffusing portion was 19.93, while that of the portion which diffused more slowly was 20.01.

The invention by Dr. Hampson of an apparatus by means of which it is possible to obtain liquid air at small expense and with little trouble placed a new instrument in our hands; and Dr. Travers and I prepared 15 liters of argon from the atmosphere, with the purpose of distilling it fractionally, after liquefaction; for we knew, from the researches of Professor Olszewski of Cracow, who has done so much to determine the properties of liquefied gases, that argon could be liquefied easily by compressing it into a vessel cooled by help of liquid air. And, moreover, we were in hope that by fractionating the air itself, gases of even higher atomic weight than argon might possibly be obtained. Both expectations were realized; on distilling liquid argon, the first portions of gas to boil off were found to be lighter than argon; and on allowing liquid air to boil slowly away, heavier gases came off at the last. It was easy to recognize these gases by help of the spectroscope; for the light gas, to which we gave the name, *neon,* or 'the new one,' when electrically excited emits a brilliant flame-colored light; and one of the heavy gases, which we called *krypton,* or 'the hidden one,' is characterized by two brilliant lines, one in the yellow

and one in the green part of the spectrum. The third gas, named *xenon*, or 'the stranger,' gives out a greenish-blue light, and is remarkable for a very complex spectrum, in which blue lines are conspicuous.

Although neon was first obtained by the fractional distillation of argon, it was afterwards found convenient to prepare it direct from air. The torpedo-compressor, which is used for compressing the air before it enters Dr. Hampson's liquefier, was made to take in the air which had escaped liquefaction in the liquefier; the denser portions were thus liquefied, and the lighter portions were liquefied by compressing them into a vessel cooled by the denser fractions, boiling under reduced pressure, and consequently at a specially low temperature. This liquefied portion was again fractionated, and yielded neon; and it was not long before we discovered that helium was also present in the mixture. The presence of helium in atmospheric air had previously been noted by Professor Kayser of Bonn, and by Professor Friedländer of Berlin, on submitting the spectrum of argon to a searching examination.

The purification of this mixture of neon and helium from argon, although a lengthy process, was not attended by any special difficulty. It was accomplished by repeated distillation, the lighter portions being always collected separately from the heavier portions, and again distilled by themselves. But after this separation had been accomplished, we found that we were unable by means of liquid air to liquefy the mixture, or indeed any portion of it. We effected a partial separation by diffusion; but it is not possible to separate by this method two gases of which the quantity is limited. Another attempt was made by dissolving the gases in liquid oxygen, on the supposition that neon might prove more soluble than helium; but without satisfactory results. It was evident that a lower temperature than that possible by help of liquid air was necessary.

Professor Dewar had by that time succeeded in producing liquid hydrogen in quantity, and had indicated the principle, which is identical with that of Dr. Hampson's air-liquefier, although he has not published any detailed account of his apparatus. Dr. Travers undertook to investigate the subject; and after four unsuccessful trials, he made a liquefier, with the help of Mr. Holding, the laboratory mechanician, by means of which a hundred cubic centimeters of liquid hydrogen could be easily and cheaply produced. There was then no difficulty in effecting the separation of neon from helium; for, while neon is practically non-volatile, when cooled by liquid hydrogen, remaining in the state of solid or liquid, even that enormously low temperature is not sufficient to convert helium into a liquid. Hence the gaseous helium could be pumped away from the non-gaseous neon, and the latter was obtained in a pure state.

The residues obtained from the evaporation of about thirty liters of liquid air, after being freed from oxygen and nitrogen, were liquefied by help of liquid air, and fractionated from each other. The separation offered no special difficulty, but was long and tedious. It soon appeared that when most of the argon had been removed, the residue solidified when cooled; but while it was possible to remove the krypton by pumping, for it goes into gas slowly even at the low temperature of liquid air, very little xenon accompanied it; for at that temperature, xenon is hardly at all volatile.

Having finally separated the gases, their densities and other properties were carefully determined; and it was also proved that they are like argon and helium, in as much as their molecules consist of single atoms. Neon, as was expected, turned out to be the missing link between helium and argon; the atomic weight of krypton was found to be 81.6, and that of xenon, 128. The volumes occupied by equal numbers of molecules of the liquefied gases were determined; and also the boiling-points and melting-points of argon, krypton and xenon. These figures are shown in the following table:

	Helium.	Neon.		Argon.	Krypton.	Xenon.
Density of gas..........	1.98	9.96		19.96	40.78	64.0
Atomic weight..........	3.96	19.92		39.92	81.56	128.0
Density of liquid......	0.3 (?)	1.0 (?)		1.212	2.155	3.52
Boiling-points...........	—	—		—186.1°C.	—151.7°C.	—109.1°C.
Melting-points..........	—	—		—187.9°C.	—169.°C.	—140.°C.
Critical temperatures..	—	—		—117.4°C.	-- 62.5°C.	+ 14.75°C.
Critical pressures.......	—	—	(Metres.)	40.20	41.24	43.50
Refractivity of gas.....	0.124	0.235		0.968	1.450	2.368

In every case there is seen what is termed periodicity; that is, a gradual alteration with rise of atomic weight, of the densities of the liquids, of the melting-points, of the boiling-points, and of the retardation of light when passed through the gas.

Let us consider, in conclusion, the position of these elements in the periodic table; and it will be sufficient to confine our attention to the groups of elements which form the neighboring columns. The atomic weights are given in round numbers.

Hydrogen.	Helium.	Lithium.	Beryllium.
1	4	7	9
Fluorine.	Neon.	Sodium.	Magnesium.
19	20	23	24
Chlorine.	Argon.	Potassium.	Calcium.
35.5	40	39	40
Bromine.	Krypton.	Rubidium.	Strontium.
80	82	85	87
Iodine.	Xenon.	Cæsium.	Barium.
127	128	133	137

It is evident that these new elements fall into their natural places between the strongly electro-negative elements of the fluorine group, and

the very electro-positive elements of the lithium group; and that, in consequence of their lack of electric polarity, and their inactivity, they form, in a certain sense, a connecting link between the two. It is curious, too, to notice that iodine, xenon, cæsium and barium form the ends of their respective columns. It is, of course, not impossible that other elements may be discovered, possessing similar properties, and yet higher atomic weights than these; but as yet there is no clue to guide us where to search for them.

It is difficult, owing to the impossibility of effecting a complete separation of the inactive elements from each other, to do more than hazard a guess as to their relative amount in air. As they are easily separated from the other constituents of air, there is no doubt as to their total amount; air contains 0.937 parts of argon and its companions by volume in 100 parts. Perhaps the table below may be taken as affording some indication of their relative amounts. Air contains by volume:

0.937 parts of argon per hundred.
One or two parts of neon per hundred thousand.
One or two parts of helium per million.
About one part of krypton per million.
About one part of xenon per twenty million.

It is of course not impossible that xenon may contain an even smaller proportion of a still heavier gas; but it is unlikely. Sea-water sometimes contains a grain of gold per ton; that is one part in 15,180,000; a grain of xenon is contained in about four hundredweights of air.

The problems suggested by the periodic table are by no means solved by the discovery of these aerial gases; but something has been done to throw light upon one obscure corner of the field. The gap between the electro-positive and the electro-negative elements has been bridged.

DISCUSSION AND CORRESPONDENCE.

A VIKING PHILOSOPHER.

In the minds of most men the name of Adolf Erik Nordenskiöld is connected with the voyage of the *Vega*, and with that only. That is a good title to fame, for his circumnavigation of the Old World, the forcing of the northeast passage, attempted in vain for over three centuries, was an exploit worthy to rank with those of Vasco di Gama and Maghelhaëns. But Nordenskiöld was a good deal more than a great explorer, and whatever he might have accomplished he would always have remained a singularly interesting character.

The doer of some striking deed soars for a space to the zenith of popular favor, and his fall is often the greater when ousted by the next darling of the public. But Nordenskiöld, from the day he entered Sweden, banished from his native Finland by the Russian government for an over-pointed after-dinner speech which he declined to withdraw, to the day when he died full of honors from all nations, was ever a hero of the Swedes, the one man whose features and fame were known in every village of the land. Fifteen years after the return of the *Vega* I crossed Sweden in his company. The lake steamer on which we set foot was speedily dressed with flags from stem to stern; as we paced the railway platform, folk turned to point him out to their children; an apothecary into whose shop we stepped drew us into his parlor to point with pride to a medallion of the hero hung in the place of honor; even a drive with him through the streets of Stockholm, where his presence was familiar, was not without embarrassment. Those who knew Nordenskiöld can understand this easily. He impressed the popular imagination like some grand mysterious figure of the Middle Ages. Rarely did man so combine the profound research of the student with the decisive energy of the geographical explorer, the remote and even fantastic speculations of the philosopher with the business-like ability of a prudent organizer, the absent-minded reverie and complete absorption of the recluse with the wide sympathies and practical readiness of a liberal politician. These broad outlines of his character were obvious to all, and manifest too in his outer person. The deep-set far-away eyes and the furrowed forehead above the shaggy eyebrows proclaimed him a seer of visions and a diver into nature's secrets, while the hard lines of the mouth and prominent underlip told of an obstinate patience joined to a fiery Viking temper; the bowed shoulders of the bookworm, voracious of fusty manuscripts in the dark recesses of a library, were belied by the firm elastic tread of the sailor and mountaineer.

The things he did and the things he said were striking in themselves, but they were the outcome of his yet more striking personality. People talked of Nordenskiöld's luck. He had the luck of all who lay the foundations of their plans deep, who make every preparation suggested by learning and experience, who know how to wait for the fitting moment, and who have the boldness to go ahead unswervingly when the opening appears. It was the exhaustive detail of his plans for the northeast passage that awoke the admiration, and gained the support, of King and people; it was by forethought, and not only by daring, that he brought the *Vega* and her consorts

from ocean to ocean, unscathed and without the loss of a single man. It was by readiness and prompt decision that he steered the *Sofia* to what, but for the Englishman, Parry, had then been the farthest north, and that on another voyage he burst the icy barrier of southeastern Greenland, which had defied assault for three hundred years.

These expeditions to Greenland were inspired largely by his desire to see the remains of the ancient Österby, the settlement of the Norsemen, an inspiration as much sentimental as scientific. On the other hand, his early voyages to Siberian waters, though not unfruitful of scientific results, were as grossly commercial as those of his fellow-pioneers, Captains Carlsen and Wiggins. But mere trade would not have taken Nordenskiöld to the mouth of the Yennissei, and we believe that in the night-watches there ever loomed before him the shadow of Tchelyuskin, the cape that he would be the first to double.

As keeper of the minerals in the State Museum at Stockholm, Nordenskiöld had to deal with objects that may be thought petty in comparison with his famous exploits. But the professor was a poet, always seeing the greater in the less, and thus it was that the dust falling on Arctic snows through the long night was for him a message from other worlds than ours, a suggestion of some primeval harbinger that brought to a cooling planet the germ of all life. So, too, a prolonged study of cracks in granite, to which his attention was first directed on Spitzbergen, led him, by a process of reasoning too complicated for repetition here, to the belief that they must penetrate to a depth of thirty to forty meters below sea-level and no further, since there they would meet with a system of horizontal cracks. Water would sink through the first set of cracks to that depth, and there would form a constant source of supply. The theory was proved correct by the diamond drill, and from it wider consequences of geological import inevitably result. But the practical benefits, especially in the large granitic areas of Sweden and Finland, are no less, and at Nordenskiöld's instigation large numbers of bore-holes have now been sunk, lighthouses on seagirt rocks furnished with a never-failing spring, and factories supplied with pure water previously obtainable only at great expense. 'Nordenskiöld's wells' will soon be household words, and they who understand neither mathematics nor geology know at least that like the prophet of old he has brought forth water from the stony rock.

But it is not my purpose to discuss the scientific labors of Nordenskiöld, so much as to illustrate his personality. Stern and reserved in appearance, he was often so in reality, but this arose rather from his abstraction in deep problems than from any aloofness of nature. He was not high-minded, however proud his looks, and could unbend without a trace of condescension. He was not a good speaker, but he was an inveterate one, and, as we have seen, his freedom of youthful speech cost him his post and his native land. On the triumphal homeward voyage of the *Vega*, there were banquets at every port of call, and Nordenskiöld, who of course spoke, employed always the language of the country. It was his custom. Even in Japan, after a few weeks' stay, he replied to the toast of his health in Japanese. The speech was not reported.

Wherever he went he collected objects of interest, and the collection he made in Japan was characteristic. He bought up all the books and manuscripts he could lay hands on, and so it is that there now exists in the Royal Library at Stockholm perhaps the finest collection of Japanese literature in Europe. The catalogue by Professor Rosny, of Paris, is well known to Orientalists.

Nordenskiöld was also a voluminous

writer. He was perhaps too fertile in ideas to have the lucidity that makes the good writer. Many of his sentences struggle through ponderous verbiage only to die in the folds of an ambiguous anacoluthon. But a picturesque phrase sparkles out here and there, as when in reference to some modern geological theories, he says, 'still the student of science is groping here, like a child after the silvery disc of the moon.' A sentence that an unkind critic might apply to some of the philosopher's own speculations.

Further illustrations of Nordenskiöld's many-sided character might be drawn from aspects of his life here scarcely referred to. Enough has been said to render intelligible the hero-worship of the Swedish people. Is the world too old for a Nordenskiöld myth to be possible? I doubt one is even now in the making among the remote homesteads in Scandinavian forests.

F. A. B.

AN ITALIAN IN AMERICA.

Professor Angelo Mosso, the genial physiologist of the University of Turin, has written a pleasant and plausible little book about America, which has been praised in various places. He duly pats us on the back and tells of our strong and weak points. 'Hurry up' is our national motto, and we are a rampant plutocracy. We make inventions, but democracy is hostile to pure science. Our neurasthenic tendencies are duly described as also our 'spoils system.' Religious sentiment is growing, and we are turning towards the Roman Catholic Church. Our universities are not progressing, owing to sectarian control. All this will be found in the book; but perhaps it is scarcely fair to quote it, as there is much there besides. Now how does Professor Mosso know us so well? He spent a month or two here on the occasion of the decennial of Clark University, and though he can not understand an English sentence, he saw us from the windows of the railway train. The writer of the present note had the pleasure of meeting Professor Mosso when he was here. He threw his arms about him in a warm embrace. Then he produced a slip from his pocket and proved by documentary evidence that there were two Americans whom he should kiss on both cheeks and about a dozen whom he should cordially embrace. Professor Mosso said later that he wished to write a book about American universities. It was explained to him that midsummer was an unfortunate time, the only university carrying on its sessions being Chicago. He asked: "Where is Chicago? Is there a university there?" The position of Chicago on Lake Michigan was described. He then said: "Can I see the University of Chicago to-day and be back in Worcester this evening?" These little anecdotes are told in the most kindly spirit by one who really admires Professor Mosso. But I must protest against his argument that there is no fundamental difference between an American and an Italian.

K.

SCIENTIFIC LITERATURE.

PHYSICS.

'The Proceedings of the Paris Congress of 1900 on Methods of Testing Materials' have just appeared in three large folio volumes. The first article by M. Ricour has for its title 'The Molecular Constitution of Matter,' but is largely devoted to the discussion of the laws of attraction and to the properties of the ether of space. This ether he imagines to be a sphere whose radius, though great, may perhaps be ultimately found. The distance of our sun from the center of this sphere of ether he finds to be such that light would require 140,000 years to traverse it. The density of the ether he finds to be such that a mass equal in size to our earth would be equal to about one kilogram. While much of the work rests upon hypotheses, the article of Ricour is interesting as showing how the engineer as well as the physicist finally comes to the ether as the ultimate source of all energy. If the ether is really a limited sphere, as he supposes, the surface of that sphere forms an absolute limit to our knowledge; for should other spheres of ether exist no waves can pass across the empty spaces that separate them from that sphere which contains our universe.

MOSQUITOES AND MALARIA.

Dr. Leland O. Howard, the entomologist to the United States Department of Agriculture, has just given us a little book on 'Mosquitoes' (McClure, Phillips & Co.) that is peculiarly well timed and important. Dr. Howard has studied his subject for many years; primarily, in the past, to devise methods for the local control of an annoying pest; but recently, since the relation of some of the genera to the spread of malarial and other febrile diseases has been established, to ascertain their peculiarities of habit and development.

Public interest in the newspaper and magazine accounts has been so marked that a book like this which gives in concise form and with scientific accuracy just what is known of the relation between disease and insects must be especially useful to correct the misinformation gaining currency among the general public. Dr. Howard gives us in detail the life history of our commonest species; both *Culex*, which is an annoyance merely, and *Anopheles*, which is the intermediate host for the organism producing malarial diseases in man. The breeding places of each are discussed and the measures which may be adopted to do away with them. A very complete account is given of the experiments which link the *Stegomyia fasciata* with yellow fever, and a brief statement shows the relation between *Culex ciliaris* and Filariasis. A chapter on classification shows fairly well what is known of our American species and gives some indication of what yet remains to be learned.

THE PROGRESS OF SCIENCE.

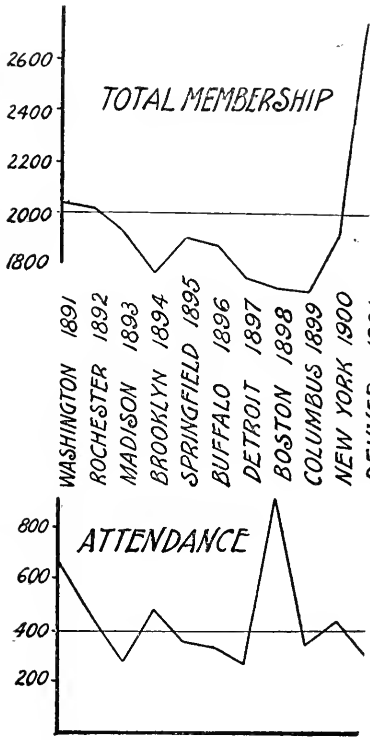
THE AMERICAN ASSOCIATION.

THE meeting of the American Association for the Advancement of Science held at Denver during the last week of August was of more than usual significance. For the first time in its history the Association met west of the banks of the Mississippi. California was ceded to the United States in the same year in which the Association held its first meeting, and the great western half of the country and the Association representing science in America have developed together. It was Frémont, then a scientific man engaged in scientific surveys, who saved California for the Union. The western States—dependent on railways, mines and modern agriculture—are the children of science. Having attained through science their remarkable material development, they are now prepared to unite with the older culture of the east in efforts for the advancement of science. Extending from the Mississippi river to the Pacific coast we have the first civilization based definitely on science, and we may expect to see in this region the world's chief centers for the diffusion and advancement of science. The first meeting of the American Association in the west is merely an announcement of what has been accomplished already, yet it represents an epoch in the history of science and of civilization.

THE meeting at Denver was itself full of interest. Though not quite so large as meetings on the Atlantic seaboard, it was larger than the recent meetings at Madison and Detroit, and nearly as large as the meetings at Springfield, Buffalo and Columbus. Further, the 306 members in attendance were mostly scientific men, as is shown by the fact that two hundred and twenty papers were presented. The people of Denver did everything possible to ensure the social success of the meeting, and the scenery and resources of the State of Colorado were of the greatest possible interest to all visitors. The address of the president, published above, was worthy of the occasion, and many interesting papers were read before the different sections. In several respects the business transacted was of importance in the history of American science. The committee on the journal, 'Science,' made its first report on the arrangement made at the New York meeting last year, in accordance with which this weekly journal is sent free of charge to all members of the Association. It appears that the fees of new members were sufficient to defray the cost of sending 'Science' to all members of the Association, and that the plan has proved acceptable on all sides. Another important step was the perfecting of the affiliation of the special societies with the Association. Hitherto the national societies devoted to the special sciences have met informally with the Association; hereafter they will be an integral part of it, being represented on the council. The council will thus become the body chiefly responsible for the organization of science in America. The Association planned for a winter meeting to be held at Washington a year from next January. Attention has already been called here to the movement now progressing for the establishment of a convocation week for the meetings of scientific and learned societies. It is now assured by the action of our leading universities and of the American

Association that this week—that in which the first day of the new year falls—will hereafter be devoted to the purpose designated. The meeting of the American Association next year will be at Pittsburg at the beginning of July, and will be presided over by the great astronomer, Professor Asaph Hall. It will undoubtedly be large and important; while the meeting at Washington will probably be the greatest scientific congress ever held in America.

There was published in this Journal for July last an article on the American Association for the Advancement of Science calling attention tothe great importance and responsibility of this institution for the development of science. Trusts and trade unions are an integral part of our present civilization, and it is our duty not to protest against them, but to direct them for the common good. Those interests that are most important for civilization should have the strongest organization, and it is gratifying to find that under the auspices of the American Association a union is being effected that will adequately represent the scientific interests of the country. There was a period of disintegration when the development of the special sciences required the formation of special societies, but we are apparently now in the midst of a movement toward such a concentration of authority as will not interfere with local autonomy. Herewith is given a curve showing the total membership of the American Association and the attendance at the meetings since that in Washington in 1891, when the membership reached its maximum. It will be noticed that there was a tendency for the membership gradually to decrease, broken only by an accession at the large Brooklyn meeting of 1894. The curve, however, rises in a remarkable way for the New York and Denver meetings. This has doubtless been largely due to the arrangement with 'Science,' mentioned above, and to the efficiency of the present permanent secretary, Dr. L. O. Howard, in bringing the desirability of membership in the Association before the scientific men of the country. These, however, are only incidents that have hastened the development of a movement demanded by modern conditions.



MORTALITY STATISTICS.

A bulletin issued from the census bureau at the end of August gives vital statistics of more than usual interest. The death rates of 1900 and 1890 are compared both as regards different regions and as regards different causes of death. It appears that careful registration of deaths is undertaken in ten States and in a large number of cities, including about twenty-nine million of the inhabitants of the country.

The statistics disclose the very gratifying fact that in ten years the general death rate has decreased from 19.6 per thousand to 17.8. This remarkable decrease is in the cities, where the rate has fallen from 21 in 1890 to 18.6 last year. The rate in the country has been about stationary, having been 15.3 in 1890 and 15.4 in 1900. This extraordinary decrease in the death rate of cities has been due chiefly to improved hygienic conditions. In the country a corresponding gain has not occurred. We may perhaps look for it in the course of the next ten years, though there is of course less room for improvement. New York City has one of the best records of progress, its death rate having decreased in ten years from 25.3 to 20.4, making the city in spite of its crowded tenement districts as healthful as Boston and decidedly more healthful than Philadelphia, in which city the death rate has remained practically stationary. But there is room for further progress in our eastern cities. Chicago has a death rate of only 16.2, and nearly all the cities of the northern and central States have a low death rate, Minneapolis and St. Paul, for example, having the incredibly low rates of 10.8 and 9.7, respectively. The most unfavorable conditions are in the south, the death rate of New Orleans, for example, being 28.9, an increase since 1890; and that of Charleston, 37.5, about the same as ten years ago. Almost as interesting as the decrease in the death rate is the decrease due to certain special diseases. The following table deserves to be quoted in full. It shows the death rate due to certain diseases per hundred thousand of population in the registration area in 1900 and 1890 together with the increase or decrease in the rate.

Causes.	Death Rate.		Increase.	Decrease.
	1900.	1890.		
Consumption	190.5	245.4	......	54.9
Debility, atrophy	45.5	88.6	......	43.1
Diphtheria	35.4	70.1	......	34.7
Cholera infantum	47.8	79.7	......	31.9
Bronchitis	48.3	74.4	......	26.1
Convulsions	33.1	56.3	......	23.2
Diarrhœal diseases	85.1	104.1	......	19.0
Croup	9.8	27.6	......	17.8
Typhoid fever	33.8	46.3	......	12.5
Dis. of the brain	18.6	30.9	......	12.3
Malarial fever	8.8	19.2	......	10.4
Unknown cause	16.8	24.6	......	7.8
Inflammation of the brain and meningitis	41.8	49.1	......	7.3
Hydrocephalus	11.0	15.4	......	4.4
Dropsy	6.9	10.3	......	3.4
Whooping cough	12.7	15.8	......	3.1
Paralysis	32.8	35.5	......	2.7
Scarlet fever	11.5	13.6	......	2.1
Septicæmia	10.0	7.7	2.3	......
Diabetes	9.4	7.5	3.9	......
Pneumonia	191.9	186.9	5.0	......
Premature birth	33.7	25.2	8.5	......
Old Age	54.0	44.9	9.1	......
Cancer	60.0	47.9	12.1	......
Heart disease	134.0	121.8	12.2	......
Apoplexy	66.6	49.0	17.6	......
Influenza	23.9	6.2	17.7	......
Dis. of the kidney	83.7	59.7	24.0	......

This table shows that consumption is no longer the most fatal of diseases, pneumonia having taken its place. Deaths from consumption have decreased over 20 per cent., while a greater relative decrease is recorded in the case of diphtheria and other diseases. The diseases that show an increase are chiefly those incident to advanced age, death from old age itself showing an increase of 20 per cent.

ARCTIC EXPLORATION.

The steamship *Erik* has returned, bringing welcome news of Lieutenant Peary. It appears that he has succeeded in rounding the limit of the Greenland Archipelago, probably the most northern land, and has reached the highest altitude yet attained in the western hemisphere (83° 50′). Mr. Robert Stein and Mr. Samuel Warmbath were picked up by the *Windward*, but there is no news regarding Captain Sverdrup. During the present autumn Lieutenant Peary expects to make explorations and in the spring of next year again to make the attempt to pro-

ceed as far north as possible. In the meanwhile, Mr. Baldwin is making a similar attempt from another direction, and there are a number of other expeditions in the far north, Baron Toll, who started from Russia in May, 1900, was recently in the Strait of Tarmour, while from the same country, Admiral Markaroff is testing his ice-breaking steamship. From Norway, Captain Sverdrup on the *Fram* has for three years been making explorations about Greenland and west of Ellesmere land. From Germany, Captain Banandahl was, when last heard from, advancing north from Spitzbergen. None of the expeditions in the north are of the same scientific importance as the national antarctic expeditions of Great Britain and Germany, but it seems certain that the next year will add greatly to our knowledge of the unknown regions of the north as well as of the south.

SCIENTIFIC ITEMS.

Professor William Theodore Richards, of Harvard University, has declined a call to a newly-established research professorship of chemistry in the University of Göttingen. It is a special compliment to the United States that Germany should seek here a professor for such a chair, especially when we remember the large number of chemists that are trained at the German universities.—On the application of the Government of Victoria, Australia, for a director of agriculture, officers of the U. S. Department of Agriculture have recommended Professor B. T. Galloway, chief of the Bureau of Plant Industry, and Professor Willett M. Hays, agriculturist of the Minnesota Experiment Station.

The Reale Accademia dei Lincei of Rome has elected eight foreign members, including from the United States Edward C. Pickering, director of the Harvard College Observatory; Samuel P. Langley, Secretary of the Smithsonian Institution, and Chas. D. Walcott, director of the U. S. Geological Survey.—The Veitch silver medal has been awarded to Mr. Thomas Meehan, of Philadelphia, 'for distinguished services in botany and horticulture.' Mr. Meehan is the third American on whom this medal has been conferred, the others being Professor Charles S. Sargent, of the Arnold Arboretum, and Professor Liberty H. Bailey, of Cornell University.

Professor Ed. Suess, the eminent geologist, gave on July 13 a formal lecture to his present and former students on the occasion of his retirement from the chair of geology. He has reached his seventieth year and his forty-fourth year as a university teacher.—Dr. Ernst Mach, professor of philosophy in the University of Vienna, has been compelled by ill health to retire from the active duties of his professorship.—Professor E. Haeckel, of Jena, has made public the announcement that owing to the state of his health, his advanced age and pressure of work, he will not in future make any public addresses or attend any scientific congresses.

A royal commission has been appointed in Great Britain to study the relation of bovine and human tuberculosis, consisting of Sir Michael Foster, Dr. Sims Woodhead, Dr. Harris Cox Martin, Professor J. McFadyean and Professor R. W. Boyce.

The British Association for the Advancement of Science held its meeting at Glasgow from September 11–18 under the presidency of Professor A. W. Rücker, the eminent physicist. The Congress of German Men of Science and Physicians is being held at Hamburg from September 22–28, under the presidency of Professor R. Hertwig, the well-known zoologist.

Science also mourns the death of the President of the Nation. We have lost a good man, representing the sterling qualities of the people. We honor a man who became great when brought face to face with circumstance. McKinley stands with Washington and with Lincoln. He who founded the Nation, he who preserved it, and he who gave it leadership, rank apart from those who lacked opportunity. The diseased brain of an assassin can not alter the course of history. When our leader falls, another is ready to take his place. But we do not forget him over whose body we advance. He is not ill-starred in his death who is honored and loved and mourned by a Nation.

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NAMES OF CONTRIBUTORS ARE PRINTED IN SMALL CAPITALS.

E.W.